THE

EDINBURGH PHILOSOPHICAL JOURNAL,

EXHIBITING

THE PROGRESS OF DISCOVERY IN NATURAL PHILOSOPHY, CHEMISTRY, NATURAL HISTORY, COMPARATIVE ANATOMY, PRACTICAL MECHANICS, GEOGRAPHY, NAVIGATION, STATISTICS, AND THE FINE AND USEFUL ARTS,

FROM

APRIL 1. TO OCTOBER 1. 1824.

CONDUCTED BY

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TO BE CONTINUED QUARTERLY.

VOL. XI.

EDINBURGH:

PRINTED FOR ARCHIBALD CONSTABLE & CO. EDINBURGH; AND HURST, ROBINSON & CO. LONDON.

1824.

P. Neill, Printer, Edinburgh.

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*** The Editor has to apologise to Correspondents for the non-appearance of their communications in the present Number; the want of room having obliged him to delay their publication until the next Number.

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No. XXII.

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EDINBURGH

PHILOSOPHICAL JOURNAL.

ART. I.—*Observations on Electrical Theories.* By JOHN LESLIE, Esq. Professor of Natural Philosophy in the University of Edinburgh, and Corresponding Member of the Royal Institute of France.

[The following Paper is the first the Author ever wrote on a Physical Question, and will be found to contain the germs of some of his subsequent investigations. It was drawn up at Etruria, in Staffordshire, where he then resided with the late Mr Wedgwood, and bears the early date of the 20th of May 1791. The experiments on which it rests were performed with care, though on rather a small scale, and amidst a variety of avocations.

The paper, though it would have been acceptable in another quarter, was, in token of respect and gratitude, sent to the Royal Society of Edinburgh. It was read at two of their meetings, on January 2. and March 5. 1792, and reserved for insertion in the forthcoming volume of their Transactions. But other communications, of a later date, were successively placed before it; and it was finally deferred to the publication of another volume, to give room for the insertion of a paper of the late Dr Monro, on the Action of Oblique Muscles, being the substance of a lecture which he had annually delivered in the College since the year 1759.

After waiting patiently for more than two years, the author could not help feeling some indignation at such unworthy treatment. He called back this unfortunate paper, and deposited it in his

closet. About a twelvemonth afterwards he revised it, made some slight alterations, and annexed a few explanatory notes; and, in this state he has suffered it to remain ever since, designing always to resume with vigour and effect the decisive experiments planned in it. On perusing it again, after such a long interval of time, far exceeding the limits of the Horatian precept, the author, though not quite satisfied with some of the mathematical explications, is convinced of the accuracy of the facts, and train of reasoning, and of the soundness and importance of the general conclusions. These leading views and experiments have been successfully introduced by him into the Lectures on Electricity which he has given since he began to teach the class of Natural Philosophy. The memoir is now printed from the original manuscript, with merely slight verbal corrections; but some illustrative annotations are subjoined in Italics, exhibiting, under a different form, similar experiments and deductions. The author intends, however, without any farther delay, to resume those interesting electrical inquiries, and hopes to be able very soon to lay the results of his extended researches before the public.]

ELECTRICITY may be justly regarded as the most striking and attractive part of Natural Philosophy. Deriving its origin from remote antiquity, it is yet altogether of modern growth, having shot up with amazing rapidity during the course of the last half century. It wants, therefore, the ripeness and solidity which distinguish all the branches of mechanical science. In the formation of electrical theory, inferences are drawn from the most fanciful analogies; and the imagination and the senses consulted rather than the understanding. Already we are in possession of numerous and important facts; and it is time to survey, with a curious and sceptical eye, our real progress, and to ascertain whether the received doctrines of Electricity deserve the honourable appellation of *science*, or ought to be regarded as the superficial production of *fancy*.

It may, perhaps, be deemed presumption in me to treat the received hypotheses with such liberty. But, in matters of this kind, free discussion should be indulged, and opposition even encouraged; and a dispassionate inquiry promises the more success, since the enlightened and profound philosophers have in general

neglected the cultivation of this subject, or have considered it as only an amusement or a relaxation from severer studies.—At present, I shall examine the opinion commonly entertained in regard to the action of *Electrical Points*, and shall take a future opportunity to communicate some experiments and observations on other parts of Electricity.

In the communication of electrical influence from one body to another, there is evidently a certain portion of time required. Thus, if a plume of glass-threads be stuck in the prime conductor, it will gradually swell and expand as the cylinder is turned round; and if, while in this state, we make a sudden connexion with the ground, we shall perceive a gentle and slow collapse. If a lock of silk or a feather be tried in this way, the effects will be the same, only rather quicker in their production. But there are other bodies, the metals particularly, in which the electrical communication seems instantaneous, or takes place in a time shorter than the interval between our sensations, and therefore totally eludes observation. We have other instances in Physics that are exactly similar. Thus, it may be shewn that every species of motion is impressed gradually and in time; yet, in most cases, this circumstance entirely escapes our notice, though in others it cannot fail to be observed. Accordingly, forces, besides the *Accelerating* or *Retarding*, have been divided into the *Impulsive*,—a division which seems to have no foundation in Nature. But the electricians have gone a more unjustifiable length: they have discriminated substances into Conductors and Non-conductors; whereas no body has been found incapable of communicating the electrical virtue. The only difference consists in the celerity with which the effect is produced; and were conductors properly classed, it would be found, in the descending range, that the velocity of transmission diminishes by insensible shades.

To the class of exceedingly slow conductors, we must refer the atmospheric air itself; for a glass tube, which includes it, is apparently as slow in conveying the electrical virtue, as a glass-rod of the same dimensions. But air is a fluid which acquires considerable motion from the slightest impression, and this extreme volubility alone may fit it, in a certain degree, for making the

communication. The air immediately environing an electrical body becomes also similarly electrified; its particles are hence repelled by each other, and also by the body, from which, as from the centre, they stream in all directions. Thus, let a large board be covered with tinfoil to within half an inch of its edge, and the surface be made smooth. Place it perpendicular on an insulated stool, parallel with the prime conductor, and at the distance of half a foot. On working the machine, the tinfoil will continue dark; but if a knob be brought near it, it will shew considerable sparks. Hence the reason of the difficulty of performing electrical experiments in open air, especially in windy weather; hence, likewise, the great improvement has been made in machines, by covering the upper part of the cylinder with a flap of silk, and coating the conductor with a thin surface of sealing-wax

But if the air, in consequence of the successive application of its particles, be thus capable of making an electrical communication, may we not infer, with a considerable degree of probability, that, whenever a body acquires the electricity of a distant one, it derives this quality merely from the motion and transference of the intervening aërial particles? Let a pointed wire be held a foot or half a foot behind the electrified conductor, and it will be tipped in a darkened room with a lucid spangle. But, while in this situation, let an insulated sheet of glass, of paper,

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Air is constantly blown from an electrified body, whether this be in the state of what is called positive or negative electricity; or whether it is connected with the prime conductor, or with the cushion of a cylindrical machine. Thus, a whirl placed on either of these, will yet revolve always the same way, or in a direction opposite to the ends of the bent wires. In like manner, a thick tapering wire, electrified either positively or negatively, will still project a stream of air, as indicated by the turning of a small wheel of card. Hence, the explication of a seemingly paradoxical fact, that any hot body will cool faster, if kept electrified. To make this experiment in a satisfactory manner, gild a very large mercurial thermometer, having a ball perhaps of an inch and a half diameter, and a long stem bearing only 30 or 40 degrees: Suspend the instrument from an insulated stand at the distance of two or three yards from the prime conductor, with which it communicates by a silver thread. Apply the hand to the bulb of the thermometer, and heat it up 20° above the temperature of the room, and note the time it takes to fall to the middle point. Repeat the operation, and then keep turning the machine, and the mercury will be found to sink down in less than half that time.

or of tinfoil, be advanced nearer to the conductor, and parallel to it; as the edge comes gradually opposite to the metallic point, the light will be observed to vanish, and not to re-appear till the other edge has completely emerged. To what cause, then, shall we ascribe this effect? It cannot surely be asserted, that the passage of the electric fluid is obstructed by the tinfoil, since metals are allowed to be the best of all conductors. But the appearance is precisely the same, whatever be the nature of the substances interposed; and the only common property which they can have in this position, is that of intercepting the current of air.

If the tinfoil be suspended near the conductor, and the point held close behind it, the lucid spangle, though greatly diminished, may, in certain cases, be still discernible: for, in consequence of the strong current of air between the conductor and the tinfoil, the latter becomes a second conductor. This objection may be obviated by varying the experiment. Thus, let a round hole, near an inch in diameter, be made in the middle of a sheet of paper which is kept insulated at the distance of half a foot from the prime conductor, and let the metallic point be held about half an inch behind the middle of the hole; it will there continue totally dark. If the aperture be enlarged, the effect will be the same, only the point must be removed somewhat farther back; and if the aperture be increased to several inches diameter, or if the paper be brought very near to the conductor, the point will resume its lucid spangle. This experiment is so conclusive, that it scarcely needs any comment. The resistance which air meets with in passing through a narrow aperture is very considerable; and therefore a small part only of the aerial current, which diverges in all directions from the prime conductor, can flow through the hole in the paper. This experiment evinces, that not only the transmission of air is necessary to produce the luminous appearance, but that, in order that the effect be sensible, there must also be a quick succession of particles. Hence whatever tends to detain the air which has already flowed upon a body, or retards the reflux of the stream, must diminish the electrical communication. Thus, let a pointed wire be stuck through the middle of a round card of two or

three inches diameter, and made to project on the other side a quarter or half an inch. Let the point, carrying its card, be advanced towards the prime conductor till it become lucid; take off the card, and the point will shew its bright spangle, though now withdrawn to double or triple its former distance. By this little contrivance, the air remains heaped about the card, and its renewal at the point is obstructed

Nor are the properties of the point and knot radically distinct; they differ only in degree. For, fix a smooth metal ball of above an inch diameter to the end of one of the branches of

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The mode which the author commonly employs in repeating this set of experiments is simpler, and better fitted for exhibition to a large auditory. A brass ball of two inches in diameter, being fixed to the top of a glass pillar, is connected, as occasion requires, by a chain or wire, with the table and the ground. Another similar ball, of the diameter of an inch, is screwed to the end of a thick wire, of near six inches in length, and tapering to a point. Now, place the ball, with its stand, at the sparking distance, or about two or three inches before the prime conductor, and opposite to the further end. Turn the machine regularly, and the sparks will succeed each other at equal intervals. While this action is continued, grasp the tapered portion of thick wire in the hand, and bring the knob gradually nearer the conductor; no alteration of effect will be perceived till the knob has approached perhaps within two inches, when the sparks will strike it, instead of the ball. But, reverse the position of the thick wire, and, holding by the knob, present the tapering and pointed end to the conductor, at the distance of above a foot, the sparks to the ball will become less frequent, but, advancing within half a foot, they will cease altogether. In this situation, apply a finger to the point of the wire, and the sparks will strike as before; and, as often as the finger is removed or replaced, they will stop or renew.

Plant a square screen of tinfoil, nearly of the same length as the prime conductor, about half a foot in front of it. While the machine is kept working, approach the tapered point towards the screen, and the electrical sparks will now continue without alteration; but, advance the point between the nearer edge of the screen and the conductor, and the sparks will cease. Cut a hole near the middle of the tinfoil, of about half an inch in diameter, and bring the point an inch behind it; the sparks will return with their former intensity, nor will they stop till the pointed wire has passed through the hole, and advanced more than half an inch beyond it. Remove this screen, and thrust the wire a quarter or half an inch through the centre of a card, formed into a circle of three inches in diameter. The property of the point to draw off the electrical influence will, by this modification, be completely destroyed. The point must be pushed near an inch beyond the surface of the card, to enable it to recover its influence. While the tapering point is held at the distance where it arrests the sparks darted from the prime conductor, advance gradually the other hand with a similar point, and, when the interval is reduced to about half an inch, the sparking will again recommence. When the pointed ends are opposite, the interval

a discharging rod, and hold it, by means of the insulated handle, within half a foot or so of the prime conductor, and while the machine is worked, small sparks may be perceived on touching the extremity of the other branch, which is stretched to its greatest distance; or resting it on the knob of a small phial, a charge may be given. If a smaller ball be substituted, the sparks will become more vivid, and succeed each other faster. But when it is at last contracted to a point, the effects will be vastly increased.

Since the distinction, then, between the point and the knob is not absolute, may we not presume, from the former investigations, that it arises entirely from a difference in the force or quantity of the aerial current? It would thence follow, that if, by any contrivance, we can produce an equal stream at a ball, we shall give it the property for which an acute body is distinguished. Let us employ the simplest and most effectual method; that of rarifying the air by means of heat. Screw a red-hot metal ball, of about an inch diameter, to the end of one of the branches of the discharging rod, and hold it at the distance of a foot from the prime conductor; on turning the cylinder, very intense sparks may be obtained at the other branch, and the effect will, according to circumstances, be equal, or even superior, to that of a point. But, as the ball cools, the sparks will grow weaker and weaker, and at last will become hardly perceptible. If we now stick a bit of lighted taper on the end of a glass tube, and bring it close to the ball, the effect will instantly be renewed; and, by thus removing or approaching the

is the greatest, and it diminishes as they approach to parallelism. The compound point thus differs not essentially in its action from a knob.

The effect of these various modifications is merely to retard or check the flow of electrified air about a point; but, if such an afflux by any means be considerably augmented round a knob, this will approximate to the property of a point. Thus, tie a bit of lighted taper close to the knob, and it will be found to arrest the sparking nearly at the same distance as the simple point. The flame likewise will appear violently blown towards the knob. To vary this experiment, take a thick wire, 12 or 15 inches long, and terminated by a solid ball of iron, an inch in diameter, and having heated the ball to redness, advance it towards the conductor, and the sparking will cease; but, as the ball cools, it must be drawn nearer to produce the same effect.

flame, the electrical communication may be suspended or reiterated at pleasure

We have only, therefore, to investigate the action of an electrified body on the ambient air, and to discover how far the influence is modified by its shape or magnitude. What I am to offer must be regarded rather as a sketch than a complete solution. The law of electrical action at the various distances has not been ascertained †; and the theory of the motions of fluids is still very incomplete. There are certain principles, however, that may direct our investigation. The electrical action resides on or near the surface, to whose extent it is nearly proportional; and it decreases rapidly with the distance of the body upon which it is exerted.

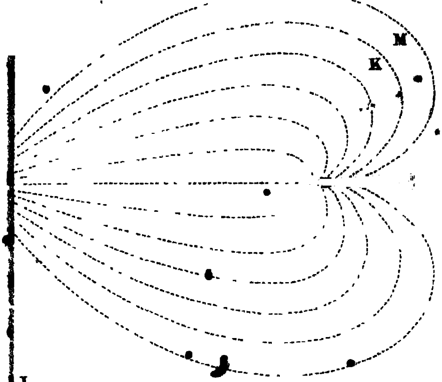
The air that surrounds the prime conductor is repelled from it in lines perpendicular to its surface; but the divergency which thus results, is rendered regular and uniform by the equal repulsion of the particles to each other. The stream flows with considerable celerity; and the particles of air, in their passage through that slow conducting medium, may reach to the distance of a foot or two, with their electricity hardly impaired by communication.

Let A (Pl. I. Fig. 1.) be a metallic ball in the vicinity of an electrified body, and N, M, L, K, &c. particles of air which have acquired the same electricity, and are repelled from the body.

It will perhaps be alleged, that this experiment admits of an explanation from the common principle, that heated bodies become better conductors of electricity. Though air, in its ordinary state, be an exceedingly imperfect conductor, yet we might infer from analogy, that, when brought to a high temperature, it will profusely transmit the electrical fluid. But this solution, though plausible, is wholly inadequate; for the lighted taper can heat only the ambient air, and its effects will therefore be confined to a very narrow sphere, beyond which the electrical communication will be as slow as ever By what magic, therefore, does the influence of this flame extend to the distance of several feet? If the electricity penetrates as slowly as before through thirty inches of air, what though its progress be rapid over the remaining inch of its track? Besides, I am much disposed to think, that the experiments performed with heated conductors are either partly or altogether fallacious, and that the electrical communication is in such cases produced by the quick renewal of air along the hot surface of the conductor.

† *The celebrated Coulomb had about this time discovered that electrical action is inversely as the square of the distance.*

Fig. 6.



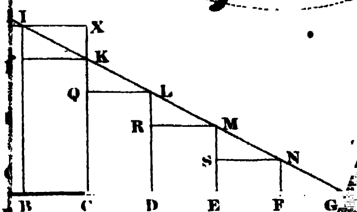


Fig. 8.

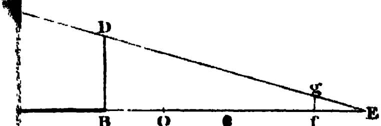
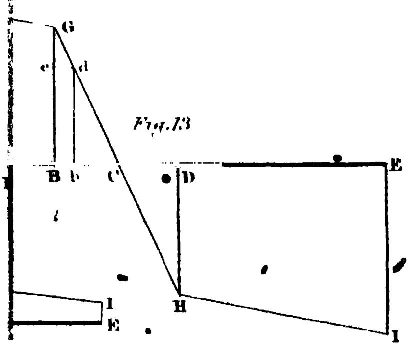


Fig. 11.

Fig. 13



The ball A will, from its mere approximation, assume an opposite electricity, and therefore attract these particles; but the action which it will exert must decrease rapidly with the distance. Let the ordinates FO, EP, DQ, &c. of the curve SRQPO, denote the forces at F, E, D, &c. Let the attraction of A have its limit at B, which is so extremely near the surface, that, on a farther approach, the particle will receive the same electricity with the ball, and consequently be repelled. The ordinate BS will therefore be the greatest; and, from S, the curve will advance fast to the line AN, which will be its assymptote.

Because the particle F tends to A with the force FO, and E to A with the force EP, E will be drawn from F with their difference XP. But if the pressure of E upon F be diminished, that of F upon G will be equally diminished; and so will that of G upon H, of H upon I, of I upon K, &c., whence all the particles will tend to A, with the force XP. Again, because E is attracted by the force EP and D, by DQ, D will be drawn from E by the difference WQ; and thus the pressure of D upon E, of E upon F, of F upon G, &c. will be diminished by WQ; and therefore these particles will tend to A with that force. Hence all the particles of the atmosphere will indiscriminately be urged to A, by the sum of the forces XP, WQ, UR, &c. or that denoted by the last ordinate BS; and the electrified particles will be moved by the additional forces expressed by the ordinates corresponding to their distances.

Such are the forces that exist previous to the motions which they produce; but a different distribution must afterwards take place. For a particle at Y adjacent to N will, in consequence of its direction towards it, be brought into the position Z, nearer to M; whence the density will be greater at M than at N, and gradually increase from N to A: wherefore M will press upon N, by the difference of the elasticity, arising from the change of density. In the same manner, L will press upon M with the difference of elasticity between L and M, and which force will be communicated to N. By pursuing this mode of reasoning, it will appear that the original tendency BS of each particle of air to the ball A, will be diminished by the amount of the condensations of all the intervening particles, occasioned by their mo-

tion; and hence the actual impelling force will gradually lessen with the distance; and, in electrical experiments performed in the common atmosphere, the flow will be found not sensibly to extend many inches. It is also evident, that the source of all this motion is the *maximum* attraction BS, which must determine both the velocity and the quantity of the aerial current.

But, besides the celerity of the stream, there are other circumstances which favour the electrical communication. The particles of air that environ the electrified body are repelled from it with considerable force, which is greatly increased by the divergency of their motion; for, as they continually become rarer, their elasticity is diminished, and consequently they are impelled by the excess of the elasticity of the succeeding particles. Hence these particles will move towards A with more velocity than other particles of air, and thus a much larger proportion of them will be brought within the sphere of the ball's action. They will then receive new accessions to their motion, from the attractive forces which are exerted upon them alone. A sort of selection even will take place; for, as the convergency near the ball is greater, the elasticity of the air will there be much increased, which, though it will, in some measure, retard the progress of the electrified particles, yet will it have a much greater effect in obstructing the approach of the others, since the impulsion of these becomes now considerably inferior to that of the others.

After the particles have successively arrived into the situation B, they will acquire the electricity of the metallic substance, and be repelled with the same forces with which they were before attracted. In consequence of the unequal actions, a particle at the distance B will press upon the contiguous one, with the difference TS of the repulsions. In the same manner, a particle at the distance C will press upon a preceding particle with the force RU, which will also be communicated to the distant portions of the air. It must follow, therefore, that all the particles of the atmosphere will indiscriminately tend from A. But this tendency is diminished by the action of another cause: for, as the particles diverge from it, they become perpetually rarer, and their elasticity weakened, and consequently they will be more and more pressed by the remoter atmosphere in an opposite direction. The cele-

rity with which the particles recede from A will therefore gradually diminish with their distance, and will become totally insensible, in ordinary cases, a few inches from the electrified ball.

But the stream which flows from A, will at first obstruct, in some degree, the motion which the affluent current has acquired. It will, however, diverge exceedingly, and soon convey its particles out of the direction of that current. There is another cause that will mightily contribute to this dispersion of the repelled particles. For we have found, that the air which flows *towards* A, is rather denser than the common atmosphere, while that which flows *from* it is somewhat rarer; and hence the affluent particles will continually press upon the refluent particles, and will gradually deflect their motion. And, if the particles be again attracted with sufficient force by the original electrified body, and return to it, their retrogade course must lie on the outside of their efflux, and envelope it.

It is indeed impossible to estimate the precise effects that will take place; the forces exerted are variously modified and combined, and the motions of the two opposite currents give an intricacy to the whole. From the preceding investigation, it appears, however, that the velocity and quantity of the aerial stream depends, not on the distance of AG, at which the body sensibly acts, but on the greatest intensity BS of the attraction. Hence it will be easy to determine the difference of effect occasioned by the shape of the body. Let P (Pl. I. Fig. 2.) be any particle placed extremely near two unequal balls, AGCI and AFBK, and acted on by their surfaces. From P, with any radius PG, describe the circles GOI and FOK on the two spheres; and from the same centre describe the other circles *g o i* and *f o k*, contiguous to the former. It is well known, that the surfaces GAI and FAK, are equal to the cylindrical rings, which have AGCI and AFBK for their circumferences, and AD and AE for their altitudes; consequently these will be as $AC \times AD$, is to $AB \times AE$, or as AG^2 to AF^2. But the nearer P is taken to A, the ratio of AG to AF, will approach nearer to that of PG to PF, and, therefore, to that of equality. Whence the space GAI will ultimately be equal to FAK; and, for the same reason, the space GAI will ultimately be equal to FAK. Consequently, the zones GOI *i o g*, and FOK *k o f*, on the surfaces of

the two spheres, will be equal *; and, therefore, since the distance is the same, they will exert equal forces at P. But a force GP will, on account of its obliquity, produce only a motion PD in the direction PC; and an equal force FP, will produce a motion PE, in the same direction. Whence the effect of the zone GOI *i o g*, is to that of the zone TOK *k o f*, as PD to PE, or ultimately as AD to AE. And since, $AG^2 = AC \times AD$ and $AF^2 = AC \times AE$; therefore AD is to AE as AB to AC; that is, the absolute effects of the corresponding zones upon the particle P, are inversely proportional to the diameters of their spheres; and, therefore, the total actions of the anterior surfaces will observe the same ratio. Again, the nearer the particle P is brought to the balls, the greater will be the disproportion between the distances of the anterior and posterior surfaces, and, consequently, the greater will be the disparity of their separate effects; and, ultimately, the forces exerted by the posterior surfaces, may be totally neglected, as not sensibly affecting the general result. It appears, therefore, that the final attraction of an electrified body of any shape, or its initial repulsion, is inversely as the radius of curvature. But it has been already shewn, that the velocity and magnitude of the aerial current is proportioned to this *maximum* force; and hence the more acute the form is, the greater will be the effect produced. In the case of an exceedingly small metallic body, other circumstances conspire; the air plays more freely around it, and as the divergency of the rays is greater, the affluent and refluent streams interfere less with each other's motions. When a large ball is used, the directions of the attracted and repelled particles of air will be nearly parallel, and their opposite forces will occasion them to stagnate about the surface, and considerably to obstruct the farther egress and ingress †.

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* *Hence it may be stated, as a simple and elegant proposition, That the space included in a circle described with the same extent of a pair of compasses on any sphere or a plane, is always the same.*

† I must confess, that I am not satisfied with these investigations; yet I cannot at present discover others which I would chuse to substitute. The various considerations involved in the problem are so undefined and complicated, as to render the solution extremely difficult.

It may be urged, that this solution supposes that a part of the surface of a body will coincide with a segment of a sphere; whereas every substance in nature is full of asperities. But it must, at the same time, be observed, that no absolute contact can ever take place; and that the nearest approach which can be made, is limited by the repulsion which the body would exert, and that, in this situation, when the asperities are minute, the forces of the various particles mingle, modifying each other, produce the same effect as if they had been disposed in the most perfect uniformity. It is thus that the rays of light are reflected from the polished surface of a mirror, with scarce any dissipation; and, for the same reason, an acute shaped metallic body will occasion, when electrified, the same celerity and extent of aerial stream, as if it had been *absolutely* smooth. Nor does the investigation require that the electrical virtue shall reside in the mere surface; it is sufficient if it be confined to an exceedingly thin external shell.

The effect will often be nearly the same, whether the point be turned towards the prime conductor, or be held in an opposite direction. In the former case, the motion with which the air flows from the conductor, conspires indeed with the action of the point, and, in the latter, it weakens that action. But as the parts of the stream proceed diverging, they become in some measure attenuated; which occasions a diminution of their elasticity, and consequently an opposite pressure. And, hence the velocity will be continually diminished, and, at a certain distance, it will become so small, that, though sufficient to renew the air about the point, it will hardly have any effect on its action. This will be found to agree with experiment *.

The foregoing principles will enable us to determine the effect of compound points, which seems inexplicable from the common notions. Let A and B, (Pl. I. Fig. 3.), be two tapered wires, whose points are placed opposite and near to each other. The

* Writers on Electricity suppose a portion of redundant fluid to collect on the surface in a thin settled stratum, which, from analogy, they term an *electric atmosphere*. This expression I have carefully avoided, as it refers to an idea essentially different from mine. Electric atmospheres have no connection whatever with the ambient air, and are believed to take place equally *in vacuo*.—But the whole hypothesis seems to be a baseless fabric.

electrified particles of air which are immediately adjacent to A and B, will be first attracted, and then repelled on both sides towards the middle C, where these contrary motions, assisted by the repulsions from the points A and B, will produce an accumulation. And, thus the increased elasticity of the particles at C will obstruct, or totally prevent, the accession of the external air, and the electrical stream will cease. When the two points are held parallel to each other, as in Fig. 4., the repelled streams will meet at C, some distance from the middle, and as this concussion will cause an augmentation of density, the afflux of the air will be somewhat checked. But the directors of the currents from A to C, and from B to C, are very oblique, and, therefore, though a part of their force be spent in occasioning a compression at C, a greater part will be employed in making the particles recede from the middle C, and consequently will admit a renewal of the stream. Accordingly, let two tapered wires be held parallel, and about a quarter of an inch from each other, as in Fig. 4., at the distance of half a foot or a foot from the prime conductor; and the points of both will be lucid, though rather fainter than if kept asunder. But if, while the points are in this situation, the wires be gradually opened till they are in opposite directions, as in Fig. 3., the light will be found to decay, and at last vanish entirely.

If the medium is denser, its motions will be more obstructed, and consequently the effect of the point diminished. Hence the improvement made on the electrometer, by including it in a small phial; for not only is it less disturbed, but the play being considerably checked, it acquires stronger electricity, and exhibits a more permanent repulsion. On the contrary, the motions will be performed with greater freedom in a rare medium, and the influence of the point will be augmented. It is thus that, in the imperfect vacuum of an air-pump, the electrical attractions and repulsions can hardly be perceived, or rather they are quickly enfeebled. Hence also the reason of the singular fact observed by several travellers, that the electrical machine works badly on the summits of lofty mountains. There is a limit, however, to this effect of the air's rarefaction: For, though the stream has its celerity and extent thus vastly increased, it also becomes proportionally attenuated, and, in the end, a smaller

number of particles will, in a given time, come into contact with the point. Accordingly, electricity is never in the least communicated through a perfect vacuum.

It is evident that the investigation which we have given will apply equally, whether the acute metallic body be originally electrified, or only be brought into the neighbourhood of an electrified substance, and whether its electricity be similar to that of the prime conductor, or similar to that of the rubber. But the form and quantity of the light exhibited on the points are different in these cases, and it is an object of curiosity, at least, to inquire into the cause of this diversity of appearance.—This will require some discussion.

The solar rays are distinguished from every other kind of matter by their passing through transparent substances, and by their suffering refraction and reflexion. But, in every case, the medium of vision must obviously possess these properties; and, therefore, all the various kinds of light are radically the same *. Nay, it is highly probable that the lucid particles which bodies in certain circumstances emit, are all derived originally from the sun: For the solar rays are partly absorbed in their passage through pellucid substances; and in such as are opaque, a still greater portion of them is lost at the surface. A quantity of luminous matter may thus be accumulated and detained in bodies, and whenever they are *disposed*, by the change of structure or any other cause, to make a partial discharge, the emission will be attended with a degree of brilliancy. There is a distinction, however, which may be remarked: The sun-beams consist of a due mixture of the coloured rays, and hence are bright white; but the ingredients of the other kinds of light are extricated in very different proportions, and from this diversity result all the various tints †.

* For all homogeneal light in passing through the same medium suffers the same degree of refraction. Therefore light is the same substance, from whatever source it proceeds. *We may state also, that every species of light must have the same, or nearly the same celerity, else it would not enable our eyes, in their present structure, to perform the function of distinct vision.*

† *Every substance whatever, while in a state of highly electrical excitement, will emit light, though this may not be always visible, since a certain duration and intensity of lustre are necessary to leave an impression upon our visual organs. Hence a metallic wire does not appear to glow during the transmission of a spark*

If we examine the subject with attention, we shall be convinced, that the only information which the eye ever conveys, is limited entirely to the quantity, the quality, and the direction of the rays which enter it. It is from the sense of feeling alone, which is diffused over the whole surface of the body, that we derive our ideas of figure and extent, of motion and force, which are the foundation of all our knowledge of matter. But from the constant habit of comparing the impressions of touch with certain indications of sight, we learn to infer the one species of sensation from the other. Nay, in most cases, the information communicated by vision serves only to suggest the other ideas. However, if a substance emits light profusely, the idea excited by the means of sight will become prominent, and occupy a principal share in the complex notion which we form. Such is the case when the substance is solid and unvaried ; but, if it be a fluid of great volubility, the faint and imperfect indications of the sense of feeling will be lost in the powerful impressions of its *brightness*. For instance, we know that heated tallow discharges inflammable gas, whose union, at a high temperature, with pure air, forms another compound, to-wit, steam. When a candle is lighted, this combination immediately takes place, and the two kinds of air, suffering a considerable change of temperature, become *disposed*, by a certain property, which is foreign to the present inquiry, to discharge a large portion of the luminous matter which they previously contained. In this case, the idea of *brightness* predominates ; we forget the prior condition of the aerial substances ; and early prejudices may perhaps suggest the independent existence of *flame*. But every body in nature can be rendered luminous by heat, by compression, or by attrition, and by the operation of all those causes which produce certain alterations of texture ; and the intensity of its brightness is ever proportioned to the quantity of change induced upon it *.

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or a shock. But if one end of a silver thread, consisting of silk, coated with the thinnest film of metal, and two or three yards in length, be tied to the neck of a large insulated brass ball, at three or four inches distance, and the other end held in the hand, such spark from the machine will be accompanied by a lucid glow over the whole extent of the thread.

* *Since this assertion was made, air, and the different gases, water and oil, have been made, by sudden compression, to emit flashes of light variously coloured.*

When glass-powder is strongly electrified, it continues to glow for some seconds; sealing-wax and rosin, spars, and other fossils, if treated in the same manner, manifest a similar property, only the appearance is of shorter duration *. If dry wood is made the conductor of a *charge*, it assumes a temporary lustre; but, if water forms the communication, it gives a sudden flash †. Thus, the duration of the brightness in these substances depends, when other circumstances are equal, on the slowness or the rapidity of transmission. The quantity of light that is emitted will be determined also by the same cause, and will therefore become extremely small when the effect is momentary; and the impression made on the organ of sight may be so prodigiously diminished, as entirely to escape observation. Consequently, in the sudden communication of the electrical virtue through continuous metallic bodies, the momentary brilliancy cannot be perceived. But air is one of the slowest conducting substances, and, if strongly electrified, it must make a profuse emission of light. Hence the brightness of the *star*, the *pencil*, the *spark*, and the *discharge*.

In these cases, it is unnecessary to recur to the supposition of an electric fluid. No argument can be adduced to prove its existence. It is founded only on prejudice, and on a fallacious inference drawn from the single and unassisted indication of the sense of sight. Some writers, indeed, are unwilling to admit the possibility of *action at a distance*, and, like the poor Indian who placed the world on the back of a tortoise, they have recourse

* It is easy to discover whether the light proceeds from the body itself, or flows either wholly or partly from the surrounding medium; for, in the former case, the surface will be distinctly defined, but, in the latter, the glare will extend a short distance all round, and gradually melt away. Thus, a ball of quartz, at a white heat, appears environed with a luminous bur, which vanishes when the contiguous air is exhausted. The particles of glass, rosin, &c., when strongly electrified, are brilliant, and nicely defined, but the metallic points communicating with a large mass are crowned with light. The reason of this will afterwards be perceived.

† A long narrow stripe of gold leaf is illuminated by the discharge of a jar, so that even metals, if sufficiently thin and narrow, such as to retard electrical communication, will appear luminous. This also shows, that bodies conduct through their mass, and not over their surfaces only.

to some intervening medium. But is it more difficult to conceive an effect produced at the distance of 1000 miles, or at the 1000th part of an inch? Or have we ever any idea of the connexion between cause and effect, but that of *constant concomitancy?* To maintain, *that no body can act where it is not*, is, in fact, to assert, that the same body can be in two different places at the same time; which is a contradiction of terms, and therefore completely absurd.

And what advantage is gained by admitting an electric fluid? All that can be demonstrated is the emission of light; and is not the difficulty increased by regarding, not the body itself, but a fluid residing in it, as the source? Nor will the motion of such a fluid in the least explain the phenomena; on the contrary, none of the effects that would necessarily result from its motion can be perceived. But, does not the profuse discharge of luminous matter from any substance constantly indicate a change of properties? And, would not an alteration, even though temporary, in the nature of an electric fluid, be inconsistent with the notions commonly received?

The various mechanical hypotheses which have amused the philosophic world, derive their origin from the early and inveterate prejudice, that all motion is caused by *impulse*. Vortices and ether enjoyed but a temporary reputation; and every sober inquirer was convinced, that such gratuitous assumptions, instead of simplifying the laws of nature, involved them in greater obscurity and perplexity. The question still recurred, what produced the motions of those elements? The understanding, bewildered and confounded amid the increasing difficulties, abandoned the pursuit: The imagination alone was delighted to dwell in mystery and illusive darkness. The chain of principles which direct this universe, terminates in the *will* of its Mighty Architect; and prudence calls us to stop where the link appears the simplest, and not to strain beyond the limits of our faculties. Many speculations have been offered on the subject of the magnetic fluid; but, unfortunately, the properties gratuitously ascribed to it, beside their absolute insufficiency, are more complex than the facts which they are intended to explain. The hypothesis of an electric fluid is so

natural, and so agreeable to the primary information of the senses, that scarce a doubt has ever been entertained of its reality. But what was presumed to be the electric fluid, is only air endued with certain properties. The existence of such a fluid is therefore illusory; it is unnecessary, and inconsistent even with mechanical principles.

If a certain change in the elementary arrangement of a body produce an *emission* of light, an opposite change may occasion an *absorption*. And, when instances of that kind occur, they will completely escape our observation; since the eye can judge of those rays only which enter itself. Nor will the brilliancy of a neighbouring body be affected; for, as the action in every case diminishes with the distance, the particles of light will be imbibed from the immediately contiguous matter, which will then be disposed to attract from the next adjacent portion, and thus, as the effects extend from the centre, they will gradually be attenuated, and become insensible. On the contrary, when there is an emission, a vast force is exerted at first, which communicates to the particles an illimited progressive motion.

On every hypothesis, it will readily be admitted, that the electricity of the cushion is the reverse of that of the conductor; for what is attracted by the one is at the same time repelled by the other. If, therefore, a body in contact with the one *emits* light, we may infer, from the contrast of their properties, that a body connected with the other is disposed to *absorb* it. The aerial streams between two adjacent bodies will consequently be affected oppositely in regard to light, and, to discriminate them, we have only to ascertain the direction of that which is lucid. The most certain and easy method is to trace the curve of their motion. The arguments commonly adduced to prove the existence of an electric fluid, may with equal propriety be employed to demonstrate the materiality of gravitation and magnetism. Nay, it might by the same reasoning, as in the infancy of science, be urged, that a moving body is actuated by a certain vital principle.

The denominations of *positive* and *negative* electricity are purposely omitted in this discourse, because they are founded on the hypothesis of an electric fluid. Let A and B (Pl. I. Fig. 5.) be two small bodies possessing opposite electricities. It is ob-

vious that the particles of air which surround A, being repelled perpendicularly by its surface, and also by each other, will flow equally in all directions. The particle projected to B will proceed, without deviating from the straight line; but a particle which receives a lateral impulse, will be perpetually deflected from its course by the attraction of B, and will therefore describe a curve that is concave towards A F. Suppose the particle has arrived at D, and that T D is there the direction of its motion; let the repulsion of A be to the attraction of B as AD to DE, then AE will denote the joint action of A and B. It is evident, that, when AD is less than DB, that is, when D lies between A and the middle perpendicular CO, DE will be much less than DB, or AE will make a considerable inclination with AB on the side O. Produce TD to G, and draw GH parallel to AE, such that DG shall be to GH as the velocity of the particle at D is to the velocity which the joint actions of A and B would, in a given instant, impress upon it: DH is the direction that will result, and G D H the angle of deflexion. Let the particle move through the space D d in the same instant, and if the repulsion at A decreases in the simple ratio of the distance, while neither the quantity nor direction of B's attraction varies, the angle D A E will receive an increment D A E Sin. $\frac{D\,d}{A\,D}$. But when B D acquires the position B d, the attraction D E will be increased, and also become less oblique to A D; on both accounts, therefore, the angular increment will be greater than D A E Sin. $\frac{D\,d}{DA}$, and this quantity will be much increased, if, what is most probable, the forces vary more rapidly. The elementary deflexion GDH is DEA Sin. $\frac{G\,H}{G\,D}$; but if the particle, in describing A D, received impulses equal to that at D, DG would be to GH, as the time elapsed in describing A D is to the given instant in which D d is described; but since the velocity of the particle increases, this ratio will be that of A D to D δ, a smaller portion than D d. Consequently, even on this supposition, the curve will bend more slowly from the initial tangent than the line A E. But the impulses which the particle receives, diminish fast in its progress from A; and

therefore $\frac{GH}{GD}$ is much less than $\frac{Dd}{DA}$, and the deflexion of the curve extremely small, compared with the angular variation of A E. Hence the inclination which the tangent makes with A E will continually increase, and when A E comes into the position A B, the curve will cut the middle perpendicular O C obliquely, and it will still continue to recede from A B, until the sum of all the little impulses A *e*, which are now exerted to better effect, is sufficient to counterbalance the force of its divergency. If the distance between the bodies is too great, the particle may never even reach B. Thus, in every case, the portion of the curve opposite to C will bend from A B, and when the curve is finite, the absciss will make a smaller angle with the origin than with its termination. The motion of the particle will indeed be retarded in its progress through the air, but this resistance will never affect its direction, and consequently the general form of the curve will remain the same; only G H will be more considerable in regard to D G, and therefore the momentary deflexions, or the elementary angles G D H, greater. Hence there are two cases in which the recession of the curve will be increased: If the particle passes through a rare atmosphere, and consequently suffers little obstruction, the attraction of B will have less influence in bending its course; or if the particle is sent obliquely at first, it will require a greater amount of opposing impulses to change its direction, and consequently will proceed farther beyond the middle before it begins to return towards A B.

Let us now suppose that A and B are two pointed bodies of some length (Pl. I. Fig. 6.) If R A and S A be perpendicular to the surface at A, it is evident that they will limit the course of all the aerial particles that are repelled from that point. Those which are emitted near the direction A B will fall variously at B; but the particles that have a more oblique projection will proceed to K and M before they bend their course inwards. At such distances, the action of the point B will be considerable, if compared with that of the large surface B and Q, and therefore the particles will tend indiscriminately to the nearest parts of that surface. But all the particles, whether they converge to B or spread over the surface L Q, were alike sent from the

point A; since a flat surface exerts hardly any action on the air immediately contiguous.

Hitherto we have considered the motion of the distant particles as affected only by the repulsion and attraction of the bodies A and B. But it was formerly shewn that the external re-action of the air surrounding a point, such as B, is diminished in consequence of its rapid influx, and therefore that the more remote portions of air are impelled towards B by the excess of the pressure. Thus, a particle M, besides being attracted by the opposite surface, will be pushed more powerfully in the direction M B; and since both this force and the attraction of the point B will increase as it advances, while the action M Q is hardly affected, the course of the particle will bend continually nearer and nearer to B: and, in the ordinary state of the atmosphere, all the particles emitted from A will terminate their motion in B, and those which pass beyond that point will be imperceptible. If, however, the air be exceedingly rare, the force arising from any change of its weakened electricity must be rapidly impaired; at the same time, the particle, suffering little obstruction, will recede to a greater distance from A B. In this case, therefore, the motions of the particles will be determined almost solely by the actions of the bodies A and B, and a large portion of the stream will flow beyond the point, and spread from B to Q.

Hence, we may determine the direction of the luminous stream: For, in rare air, the body from which the discharge is made will seem dark, except at its points; whereas, that which receives it will not only appear bright at the extremity, but beset with a bur of light, for a small part of its length. The experiment was accordingly made: Two brass-wires, about a quarter of an inch thick, and tapered, were fixed with their points opposite, and about 2 inches from each other, in a small receiver, and the air considerably exhausted; and it was constantly observed, that the wire connected to the cushion of the electrical machine was decked with luminous streaks for the space of more than an inch, but the one connected to the prime conductor was bright at the point only. The curves described by the different portions of the stream could also be partly traced; though, beyond the middle of the two points the diffusion was so great, that the light

became attenuated, and hardly perceptible. We may therefore demonstrably conclude, that a substance, which has the same electricity with the prime conductor, is disposed to *emit* light, and may infer, that a substance, which has the same electricity with the cushion, is disposed to *absorb* light.

These principles will easily explain the phenomena of the *star* and *pencil*. For let A (Pl. I. Fig. 7.) be a pointed wire, having the same electricity with the prime conductor, and B another, having the same with that of the cushion: The particles of air, contiguous to A, will be dispersed, as from a centre; but their directions will be limited, by lines perpendicular to the surface, and the densest part of the current will form a cone, whose angle is generally about 60°, or less. As the particles proceed diverging, their light will continually grow more dilute, and melt away, by insensible degrees. Beyond CD, the luminous air will be so widely spread, that its glow will be totally obscured by the general lustre. Nor will the particles become again visible at an equal distance from B; for since they flow towards that point, in all directions, they will be diffused through a surface EFG; that is, at least, sixteen times larger than the base CD, of the cone. The particles will not, therefore, discover their brightness, till they have approached four times nearer B, and, being then concentrated, they will form a lucid star.

There is a circumstance, also, which will still more discriminate the star from the pencil. It is well known, that a faintly illuminated body appears still darker, when viewed beside a bright one; and hence a stream of fading light may be traced to a greater distance, if the gradations of shade are slow. But the rate of expansion, at CD, is sixteen times slower than at the equal surface OPQ; and, consequently, though the lucid particles are barely discernible, at the surface of the sphere, they may be followed to some little distance beyond the base of the cone. Hence, also, the pencil seems to die away, whereas the margin of the star is well defined.

Perhaps the brilliancy of the aërial particles is impaired, by the light which they emitted, at the pencil. In that case, it would be necessary that the particles be still closer accumulated at B, before they become visible.

We may observe, that, if the intensity of A's electricity continue the same, and the air flow uninterrupted, the distance to which the bright cone extends, will be determined by the number of luminous particles in contact with the point A. For since the divergency is unaltered, the brightness of CD will be proportioned to the quantity of lucid matter spread through it. Hence, the pencil is contracted by making the point finer; and, in many cases, it cannot, on a small needle, be distinguished from the star.

The pencil at A will not be sensibly affected, by altering the shape and magnitude of the body B, or even by removing it entirely; for, at that distance, the action of B is quite inconsiderable. If the pointed metallic body has the same electricity with the cushion, and no conducting substance be near it, the appearance of the star is owing to another principle. The adjacent air, from its proximity alone, acquires an opposite electricity, whose intensity diminishes fast with the distance; and hence the same effects, almost, are produced, as when the aërial particles are emitted from another body.

If the pointed wires, A and B, possessing opposite electricities, be brought near each other, the cone will become narrower, and extend; the anterior part of the star will also protrude, while the rays will gradually bend more from behind, till the light forms a continued stream between the two points; and thus a slow, though constant, agitation is communicated to the air, which will occasion a faint whistling sound. If two metallic balls are placed at a short distance apart, and connected, the one to the conductor, and the other to the cushion, while the machine is worked; the aerial stream will play faintly between them, their electricities will become more intense, and their actions will increase, till a violent impression overpower the resistance, and produce a quick discharge, which is not renewed until there is an equal accumulation of the electrical virtue. And, hence, the sudden concussion causes a sharp, loud sound. The cylinder of luminous air between the balls, by reason of the repulsion of its particles and their slight divergency, is protuberant and faint, or purplish at the middle; and, as its motion is obstructed by the opposite dark stream, it makes sudden deflexions and contorsions from its course, and occasions that forked appearance, which always obtains, in a certain degree. In dense

air, a greater resistance must be overcome, and consequently the spark will be shorter, and the sound louder; but if the air be rarefied, the effect will take place, though the balls are farther asunder; and if, while they are in this situation, it be still more attenuated, the streams will play faster, and the light grow more vivid. A certain limit will, however, be attained; for the quantity of air that serves to communicate the electrical virtue may be so much diminished, as to counteract any advantage from its rapid motion, which can never pass certain bounds; the appearance will, therefore, become fainter and fainter, and, in a perfect vacuum, the electrical light will totally vanish. If sparks be sent, about the distance of 18 inches, between two balls, in a glass cylinder, nearly exhausted, the streams of luminous air will seem to play in opposite directions, much attenuated, however, near the middle.

The principles now investigated, will explain various other phenomena. Thus a point, projecting from a broad metallic plate, that is held at some distance from the prime conductor, will be entirely dark, if placed in the middle of the surface, but will become luminous, when it is removed near the margin. A sheet of tinfoil is bordered with light, but the rest of its surface, however rough or ragged, is totally obscure. Though a strip of half an inch, or even an inch, be cut out from the centre, the edges so formed will remain dark; but a narrow slit made near the verge will be distinguished by its brilliancy. For, in these instances, the air is almost stagnant over the surface, and flows off, bending by the edges.

But, whatever speculations we may form in regard to electrical light, and the mode in which the point and the knob produce their different effects, we must admit that the electricity is never communicated, in any perceptible degree, to a remote and unconnected body, but by means of a current of air; and this established principle will enable us to estimate the real effects of conductors or thunder-rods.

When two portions of air, near the point of saturation, and of different temperatures, are mixed, a quantity of the dissolved vapour is precipitated, and resumes its aqueous state. By this conversion, the mass acquires electricity; and the consequent repulsion exerted tends to disperse the minute globules of water, which will float in the atmosphere, or rather, will de-

scend with that slow motion which is sufficient to occasion a resistance on their large surface, equal to their gravitation. If the cloud thus generated, reach the ground, it will soon communicate its electricity. If it be suspended at some height, the electrified air will stream from it in all directions; and if its formation be gradual, this discharge may suffice to waste its force. But when a vast cloud is suddenly formed, the aërial emission hardly impairs its electricity; and as it is carried along, it continually approaches, by its attraction, to the surface, which assumes an opposite electricity; the air now rushes with violence, and the cloud bends faster downwards, till at last its lowest verge reaches the ground, and a total discharge is made. The magnitude of the stroke will evidently depend on the extent of the aqueous mass, the suddenness of its precipitation, and the rapidity of its descent.

The air, which streams in all directions from the cloud, is dissipated among the more remote portions, and thus gradually communicates its electricity. Hence, from the wide dispersion, owing to the distance, the electricity of the air at the surface of the earth must be weak; and, even in the midst of the storm, the electrometer is less affected than if placed only a yard behind the prime conductor. Yet the action of the thunder-rod is confined entirely to the air which immediately surrounds it, and the quantity of aërial current which it can produce, must evidently be inferior to what is directed to the point, when held several feet from the conductor of an electrical machine. But to avert the stroke, it would be necessary that the whole air between the surface and the cloud should be brought successively in contact with the top of the rod. Nor is this all; for the air will be constantly replaced by other electrified portions emitted from the cloud. The effect of the thunder-rod is therefore, comparatively, but a drop in the ocean †. It may be easily shewn, that, however pointed and tapered, it would require a thousand years to guard at the distance of an hundred yards; if terminated with a knob,

It is a common observation in the country, that, after a thunder-cloud has passed, the wind still blows from the cloud, though in an opposite direction.

† It appears, from the experiment with the heated ball, that a good kitchen-fire has more efficacy in preventing a house from being struck, than a whole magazine of thunder-rods. Hence one of the reasons why a thunder-cloud diminishes so fast in passing over a large city.

it might take ten thousand years *. Such are the vaunted performances of thunder-rods! and such the advantages of their different forms †! Nor can we appeal to experience; it never can be proved that thunder-rods have produced beneficial effects, but several instances may be cited where they have afforded no

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* This may be determined by a very simple experiment. A brass ball, 1.7 inch in diameter, and .1 inch thick, was heated to about 1400°, and held at the distance of half a foot from the conductor, for the space of 3 minutes, at the end of which it was cooled to 600°. Its effects seemed equal to that of a pointed wire held in the same situation. Hence the quantity of air which came in contact with the ball, was equal to what flowed upon the point. Let m denote the mass of the ball, and x the excess of its temperature above that of the common air: then it is ascertained by experiment, that a diminution of temperature dx in the brass would communicate an equal rise to about $\frac{m}{10}$ of the air, and therefore a portion $\frac{mdx}{10x}$ will be heated up to the temperature of the brass, and be succeeded by other portions of air. But $\int \frac{m\,dx}{10\,x} = \frac{m}{10}$. Hyp. Log. x + C, and C $= \frac{m}{10}$. H. Log. 1400; whence the whole air brought, at different times, in contact with the ball is $\frac{m}{10}$. H. Log. $\frac{600}{1400}$, or $\frac{m}{10}$ nearly) and consequently, in the space of 3 minutes, a sphere of air about 10 inches in diameter would flow upon the ball; therefore in 1000 years the pointed wire would exhaust the electricity from a sphere of air one furlong in diameter. I will not pretend that this calculation is entirely accurate; but if it approach even within the thousandth part of the truth, it is more than sufficient for my purpose.

† *The utility of thunder-rods not being once questioned, it was yet keenly disputed in England for several years, whether these should be terminated by* points *or by* knobs. *But, what is amusing, politics soon came to be mixed up with the controversy. The powder magazine at Purfleet, though guarded by pointed conductors, happening, in* 1778, *to be struck by lightning, the Privy Council made an application to the Royal Society to investigate the cause of this accident. A Committee was accordingly named of its ablest members, who, still adhering to the hypothesis of Franklin, only recommended additional pointed conductors to be placed at nearer intervals. This Report, in the height of the revolutionary war, could not be otherwise than displeasing to the courtiers, who, from their violent antipathy to the American philosopher, were as eager to depreciate his science as to deride his patriotism. They accordingly set on foot a subscription to enable Mr Wilson to perform electrical experiments on a large scale in the Pantheon, and the conclusions thence drawn seemed favourable to the theory of knobs. The Royal Society was in consequence desired by high authority to revise their Report; but the President, Sir John Pringle, replied with some warmth, that* he could not change the laws of nature! *This venerable person, however, being worried on all sides, soon resigned the chair in disgust, and retired into the country.*

sort of protection. Nay, we shall be convinced, that fully an equal proportion of the buildings armed with such supposed safeguards, have been struck with lightning. But if thunder-rods are useless, they are also innocent; and, that they provoke the shaft of Heaven, is the suggestion of superstition rather than of science. The cloud exerts an attraction, indeed, upon the surface of the ground, but the force depends solely on the distance, and is not, in the least degree, affected by the shape or quality of the substances below. It rolls towards the nearest and most elevated objects, and strikes indiscriminately a rock, a tree, or a spire.

If a thunder-rod be then an harmless, though idle appendage of a house, why awaken uneasy apprehensions? It might at least inspire confidence in the moments of danger; and if happiness consists merely in idea, why not indulge delicious error?—Yet, though the inevitable stroke cannot be turned aside, its destructive effects may be lessened; and an investigation of the real action of thunder will conduct us to the proper principles.

We have already seen, that electricity is never communicated from one body to another, but by the intervention of some medium. If this be extremely rare, and also a very slow conductor, such as air, the particles contiguous to the electric body may be projected from it, and those, being again succeeded by others, may thus gradually form the communication. But if the medium is either a solid, or a fluid of considerable density, which, consequently, does not easily admit an internal motion, all its particles will retain their situation, and be electrified during the whole time of the transmission. Let the uniform substance AG (Pl. I. Fig. 8.) be supposed to connect an electrified body A with a vast mass at G. During the first instant, a minute portion of the electricity is communicated to a certain distance B, such, that the little space AB becomes electrified as intensely as the body A. In the second instant, the half of this portion will be distributed to an equal distance BC; but the equilibrium which would thus be established through AC, is at the same moment destroyed, by a restoration of the electricity in AB. In the third instant, a smaller communication is made to the space CD, while BC receives a larger accession; and thus

the time of transmission will be proportional to the distance AG. But if the conducting substance be inconsiderable, compared with the body A, the contiguous part AB will retain its intensity AH, even after the electricity begins to spread at G; and since no communication can ever take place, unless each point be more strongly electrified than the succeeding one, there will evidently be established a regular gradation of intensity BI, CK, DL, EM, and NF, from A, where it is greatest, to G, where, from its wide and rapid dissipation, the electricity becomes totally insensible. Therefore the electricity contained in AB, BC, CD, &c. will now be denoted by the quadrangular spaces AHIB, BIKC, EKLD, &c. Suppose that the portion AC of the conductor is suddenly disconnected, then the electricity HIO will in one instant be transferred from AB to BC, and the equilibrium in AC restored. But the continuity of AG, though it will prevent this equilibrium, cannot affect the quantity of communication. In one instant, therefore, the electricity HIO will be imparted from A to AB, IPK, from AB to BC, KLQ, from BC to CD, &c. If AG and AB be constant, the triangle HOI will be proportional to HA; that is, while the conductor remains the same, the rate of transmission will be proportional to the intensity of the electricity in the body A; and hence, if the electricity be diminished one-half in one second, it will be reduced to $\frac{1}{4}$th in 2″, to $\frac{1}{8}$th in 3″, to $\frac{1}{16}$th in 4″, &c.; but the electricity that still remains will become soon imperceptible. If AH and AB are given, the triangle HOI will be reciprocally as AG; or, when in other respects the conductor remains the same, the rate of transmission will be reciprocally as its length. But the quantity of electricity communicated in a certain minute interval, will, when other circumstances are alike, depend obviously on the proportion of matter in A to that in the adjacent portion AB of the conductor; or, since AB is given, the intensity diminishes inversely as the matter in A, and directly as the transverse section of the conductor at A. The time of transmission will also depend greatly on the nature of the connecting substances; for these differ extremely in their conducting qualities.

The foregoing principles will likewise apply in the case of the charged jar. For let AG (Plate I. Fig. 9.) connect the two coat-

ings, and AH, GH denote their opposite electricities; the intensity of A will gradually diminish to the centre D, which is neutral, and again gradually increase with opposite electricity to G. Hence, during the transmission of the electricity from A to D, an equal quantity is also communicated from G to D; and therefore, the discharge will be performed in the same time that the electricity would be conducted from one of the coatings A and dissipated at the middle point D.

If the flexures of the conducting substance are not sudden, the time of communication will depend on its length, and not on the distance between the extremities; for the gradation of intensity is uniform in the successive portions AB, BC, CD, &c., and these may be regarded as indefinitely small chords inscribed in a curve, to which they are therefore equal.

Let AC (Plate I. Fig. 10.) be a compound conductor, of which the part BC is either narrower than the other AB, or of a slower conducting quality. Let the intensity of the electricity of A and of B be AE and BF; produce EF to meet the extension of AC in D and join FC; make the minute part A a equal to B b. Since the same quantity of electricity must be transmitted through every part of the conductor, the differences Ek and Fi between the proximate intensities at A and B, are inversely as the conducting powers of the two portions AB and BC. But Ek or Fh : Fi : : BC : BD; and the rate of communication will evidently be the same in the uniform conductor AD as in the compound one AC. This method of investigation might be extended to more intricate cases, were it judged necessary *.

To bring the various circumstances into a single point of view.

* *In the case of a chain and other interrupted conductors, the celerity of communication will depend chiefly on the surface of contact. Thus, the shock sent through a chain, stretched by a considerable weight, is sharper than through a chain only spread loose. If a dissected frog, having its thighs and its spine sheathed with tinfoil, be laid on a table, and several persons grasping each others moistened hands, while two of them, holding merely a brass chain, form a circuit; this very sensible electrometer will, after the first tremor, continue quiescent, but on giving the chain a sudden tug, the subject will be thrown into violent convulsion, which is renewed at every twitch. By applying strong pressure to enlarge the quantity of contact, the general effects may be greatly augmented in recent and very curious experiments of electro-magnetism.*

The time of transmission is directly as the length of the conductor is to the mass electrified, and as the logarithm of the ratio of its intensity before and after communication *; and inversely as the celerity of the conducting quality, and as its transverse section. It would follow, that the equilibrium will never be perfectly restored; but in ordinary cases, the electricity will be imperceptible when diminished to the hundredth part, and the interval elapsed in the discharge is therefore between five and ten times greater than that required to reduce the intensity to one-half.

We have hitherto conceived the conducting substance to connect both coatings of the charged jar at the same instant; but, in ordinary cases, the one end is made to touch the exterior coating, while the other end is brought to touch the interior coating. Let AB (Plate I. Fig. 11.) represent the conductor communicating with one coating A, and the extremity B held near the other coating O. Then, as fast as B is electrified, the contiguous stratum of air will acquire electricity to the same intensity, and be succeeded by other portions of air. If the intensity of B's electricity is constant, the communication made in a given interval of time must be proportioned to the number of aërial particles in contact with B; that is, as the proximate surface of B, and the density of the air. We still suppose, that the electrified strata are thrown from B to O as quick as they are formed; if the distance BO be considerable, the communication would be somewhat retarded, and the time of discharge increased. Let AC denote the intensity of the electricity at A; BD, that at B; and conceive the surface at B, and the density of the air, to remain the same. Then a quantity of electricity C*c*, will be transmitted from A to a given distance *b*, in a given time; and this must be equal to the communication made at B, and consequently proportional to BD. Produce CD and AB to meet at E, and make E*f* = A*b*; wherefore BD : *fg* or C*c* : : BE : E*f*; but E*f* is given, and consequently BE is given. Hence the time

* I have supposed, that the *rate* of the communication of electricity is proportional to the *intensity*. Perhaps this is not strictly true, though extremely probable: It is exactly so in the application of heat. But my aim in this discourse is only to obtain an approximation.

of discharge through AB will be the same with that through AE, which terminates in a neutral point E at a certain distance, BE beyond the former, and determined by the proximate surface of B, and the density of the air, and affected in some degree by the obstruction to the current occasioned by the distance BO. The same investigation will apply, if we suppose conductors to proceed from the two coatings, and brought near each other; for the effect will be the same as if they were continuous, and a certain addition made to the length of each.

We have talked of the quantity and intensity of electricity: But such language may be held, whether we consider electricity as a substance, or as a quality only. In the same manner, we speak of the *motion* of bodies, of its communication from one to another, of its distribution, &c.; yet no person ever imagines that motion has any independent existence.

These deductions are confirmed by experiment, as far as the nature of the subject will admit. Thus, if a slip of paper, sufficient to discharge a Leyden phial in about a quarter of an hour, be rubbed slightly with charcoal dust, it will perform the effect in ten seconds. If now reduced to one-half its breadth, it will require about double the time; and, if again shortened to one-half its length, the time will be nearly the same as at first. If the charcoal dust be gradually strewed thicker, the discharge will become more and more rapid,* till the interval can no longer be distinguished. But though the sense of sight now fails us, the impression made upon the ear may direct our judgment. For such is the peculiar structure of the organ of hearing, that the loudness of sound depends not so much on the quantity of aërial impulse, as on the rapidity of the concussion.

* Compare the noise of a discharge of a file of musquetry with the sound of the explosion of a single cannon. *Pulvis fulminans* is brought to a fluid mass before it inflames, while in gunpowder the fire is comparatively slowly communicated through the contiguous grains: And though the expansive force of the former be inferior, it occasions a much sharper and louder explosion. The sound of a cannon, heard at some distance, seems not only fainter but flatter; because every intermediate point becomes the centre of new aërial undulations, which, mingling with the primary ones, prolong and blunt the impression made on the ear. For the same reason, the notes of a musical composition become mellowed in their progress through the air.

When the powdered charcoal is moderately thick, we are conscious only of a light and sound; yet, if the thickness be increased, this sound, which at first is only a faint whistling, grows dull and blunt, and by degrees more intense, till at last it becomes loud and sharp *. The same observations may be made with a thin surface of water in the bottom of a basin, by adding small quantities continually.

But in this mode of performing the experiments, there is some inaccuracy; for the contiguous portions of air become electrified, and by their flow, they assist greatly the effect of such slow conductors. Let, therefore, the water or other liquids, be inclosed in bent glass-tubes of different diameters and lengths, and equal metallic balls cemented to the ends. In this way, it will be found, that a cylinder of water, one-fourth inch diameter and five long, discharges half a foot of coating in about one-fourth of a second, with a blunt noise and deep orange light; oil of olives, treated in the same way, is slower in its effects, and produces a fainter noise. But sulphuric acid gives a much sharper sound, and the time is indistinguishable; when mercury is used, the sound is vastly louder, though greatly inferior to that which the other metals would occasion. If the discharge is made by a harpsichord wire, and by a common thick wire of the same length, the sound will be sensibly different. By extending the chain, the explosion will be gradually blunted, and even reduced to the dull noise of the water discharging rod; and any further addition to the length will now be more perceptible than before. For, when the vibrations succeed with extreme rapidity, their impressions are confounded on the ear. And thus, as quick flashes, whatever be the difference of their intervals, affect the eye alike, so sudden pulses, though of various celerity, produce the same sensation on the organ of hearing. Hence the loudness of the explosion is not sensibly different, when the

* We may also notice the quality of the light to vary. It begins with violet, and passes through the gradations of purplish, reddish, orange, yellow, and in the end assumes a bright white. *The species of light emitted, seems to depend, however, more on the peculiar nature of the affected substance than on any other circumstance. Thus an electrical discharge through carbonic acid gas appears of a brilliant white, but through hydrogen gas of a dull red. A lump of sugar exposed to a shock, glows with a green light, while a ball of wood or ivory emits a crimson, and a body of powdered charcoal throws out a yellow gleam.*

discharge is made through a thick wire two or three inches long, or through one of as many feet. Hence, also, no sensible variation is occasioned by altering the size of the ball which terminates the discharging rod; but if it be tapered to a point, the sound will become dull and obscure, and the more so as the point is made more acute.

By comparing such experiments, we shall find, that the different substances differ astonishingly in their conducting qualities. Thus, when the length, thickness, and other circumstances remain the same, the electricity is transmitted some hundred times faster through linen-thread than through fir-wood; several hundred times faster through the metals than through water, &c. Hence, also, we may understand the nature of what is called a *lateral discharge.* For electricity is alike conducted through all the substances that form the communication; only, during the given time of the discharge, the quantities sent through each are proportional to their celerity of transmission. It is thus that we scarcely feel any impression when we discharge by holding a chain: but, if we dip both hands in water that forms the communication, we receive a violent shock *.

But the most important subject is to ascertain the effects produced on the conducting substance. If the electricity is diffused uniformly through it, the internal particles will be urged by an equal repulsion on all sides, and therefore must continue at rest. Those on the surface only will be pressed outwards; but, if surrounded by air, the forces must be counterbalanced by the repulsion of the electrified aërial particles. However, if the electricity be unequally diffused, the equilibrium of forces will be destroyed. For if the particles B (Pl. I. Fig. 12.) repel C with a force, BI and D repel C with a force DL, the difference IM will tend to disunite the particle C. The total exer-

* *In farther illustration of this doctrine, it may be mentioned, that the common experiment of firing gunpowder inclosed in a quill, having a wire inserted at each end, will very seldom succeed, even with a strong electrical shock; but if the circuit be partly formed by a column of water contained in a narrow glass tube, the transmission of only a moderate charge will never fail to cause explosion. Here the mutual repulsion among the particles of the gunpowder is, in consequence of the retardation from the aqueous connection, exerted during a portion of time sufficient for commencing their chemical transformation.*

tion will therefore be equal to the amount of all the successive impulses; in the same manner as the motion acquired by a falling body is proportional to the time of descent. It is of no consequence, that, in the one case, we perceive the accumulation gradually effected, while the time required in the other is totally insensible; for the quantity must be finite in both. However, since the cohesion of the particles occasions a certain resistance, the total obstruction will also be as the duration; and hence the force which effectually tends to produce disruption, will increase more slowly than the time, just as a body falling through air or water receives, at equal intervals, smaller and smaller additions to its velocity. Nay, the reaction made by the distended particles of a solid substance may at last counterbalance their difference of repulsion; in which case the continuance of the exertion will produce no farther effect, just as, to continue the allusion, a body, in its descent through a resisting medium, acquires a certain uniform motion. And hence the incessant working of the electrical machine is insufficient to burst the prime conductor.

We can now ascertain the effect produced by a shock on different conductors. If the length is constant, the disturbing forces will be the same, and the absolute exertion will depend on the time of transmission, though in a less ratio; and will therefore be determined by the slow transmitting quality of the substance, but much more by the smallness of the transverse section. When the distance of communication is shortened, the time of action is indeed diminished, but the successive impulses on which the effect mostly depends are proportionally more powerful. It is also very probable that the repulsions increase faster than the intensity of the electricity; and consequently both causes will conspire to burst the short conductor. Suppose that AB and DE, (Pl. I. Fig. 13.), are two similar conductors connected to the two coatings of a charged jar, and the intervening part BD of the same breadth, but of a slower conducting substance. Let the celerity of transmission from A to *a* be to that through the equal distance from B to *b*, as 1 to *n*. Then, since the quantity of communication must be the same through the whole of the compound conductor, the difference of intensity, F *f* must be to G *e* as 1 is to *n*, in order that G *c d*, transmitted in the time *n*,

may balance the accession F fc in the time 1. Hence the force which tends to separate the particles of BD must be n times greater than that which disturbs the texture of AB or DE. If B D be shortened, G c will be proportionally increased; therefore B D will burst with greater force. The same reasonings will apply with little variation in the case of an ordinary discharge; for if the end A is connected, (Pl. I. Fig. 14.), to the one coating, and I is brought near a wire communicating with the other, G c will be n times greater than Ff; whence the disuniting force is always greater in BD than in AB or at E, and increases as BD is contracted. It is obvious that the same inferences may be drawn, if we suppose BD to consist of the same substance with the rest of the conductor, but only narrower. If the conductor, even though it be continuous, is surrounded about the middle with a slow conducting substance, the adjacent particles will be strongly electrified, while the remote ones will be little affected, and hence, if the force be sufficient, the substance will burst outwards: And, if the substance is an extremely slow conductor, though continued all along the proper conductor, it may be forcibly detached; for, at a distance from the source, it will derive its electricity from the contiguous matter.

The foregoing principles will explain an immense variety of phenomena. I shall notice only a few.

Hence, the discharge is luminous through water inclosed in short tubes, but never along extended wires; for in a thin cylinder of water, the electricity is too much attenuated, and, in metallic conductors, the interval is too short, to allow a sensible emission of light. A chain is indeed marked out with a lucid track, but this proceeds from the air in contact with the points of the links, which retain their electricity after the transmission is made by the metal; which might be evinced by performing the experiment *in vacuo* *

Hence, if water, oil, &c. be inclosed in a small glass-tube, and two inserted wires be brought near the middle, the tube

* *A very beautiful appearance is produced, by sending a small shock through a long iron chain, suspended from glass pillars in festoons. Bright yellow corruscations are seen to dart from the end of every link. This effect is occasioned by the oxidation of the points of the iron, and therefore ceases along any part of the chain which has been laid in water.*

bursts by a discharge, and the violence is the greater the nearer the ends of the wires are to each other.

Small bits of wood, charcoal, nitre, spars, and other minerals, are shivered or burst by a discharge; but they are never perforated. A small cylinder of clay, with wires inserted at the ends, is bilged by the shock into a globular form. When the discharge is made through a card, the part immediately under the knob is the most intensely electrified, and the difference of the proximate repulsion the greatest, and, the cohesion being small, an entire separation takes place; at the same time, the lateral particles are distended. Hence the small hole with a bur on each side, which is constantly observed. For the same reason, if a small phial be filled with oil, and a wire immersed in it, and the point brought near the side, the glass will be perforated by a spark. The same appearance is observed in spontaneous discharges.

We may likewise see the reason why the sound of a discharge through the slow conducting fluids of the body is low and flat. Hence also the sensation of the shock, which is owing partly to the general distension of the particles, and partly to the greater difference of the repulsions between the fluids and the contiguous solids Wherefore the shock is more violent when given to a single person than to a number joined in a circle; for the gradation of intensity is more marked in the former case. When the communication is made through the head, the difference of the repulsions is greatest at the membranes which envelope the brain; that delicate organ is compressed, and the stun is violent. Hence, if the action lasts for a considerable time, an immense distension will take place. In death by thunder, the bones are dislocated, the vessels burst, and the body turned black by the extravasation of the blood: the hat is rent, the shoes torn from the feet, and perhaps the heel, which is a slow conductor, is perforated. Hence also, the rocks are shivered; the trees are split, or the bark peeled from the trunk.

All these effects depend on the *duration* of the action; and the great object is therefore to diminish that time. A broad, thick metallic substance ought to form the principal communication between the thunder cloud and the ground. The best mode perhaps would be to continue the lead down from the roof

at some distance from the walls *. This would be particularly advisable in powder-houses; and though to divert or dissipate the storm is an idea wholly chimerical, we may yet lessen the real danger, and mitigate the fated stroke.

It may now be proper to give an abstract of the principles which have occurred in the course of this inquiry. Electricity is a *state* or *condition* of which every species of matter is susceptible. It is of two kinds, attended with opposite effects: The one disposes bodies to *emit* their light, and perhaps diminishes their capacity for heat; the other disposes bodies to *absorb* light, and perhaps increases their capacity for heat †. Bodies similarly electrified repel each other; those dissimilarly electrified attract each other. Substances in the vicinity of an electrified body, at least their proximate portions, assume an opposite electricity, which is the more intense, in proportion to their nearness and their extent. But to the contiguous substances, the electrified body communicates a part of its own electricity. Even a distant substance may acquire the same electricity from the successive appulses of the intervening air: And the acute form has the property of accelerating prodigiously the motion of this aërial current. The celerity with which electricity is communicated depends on the quality of the conducting substance and the difference between the proximate intensities, and is therefore the greatest through thick and short conductors. In the case of compound conductors, that celerity will be in the ratio compounded of the conducting power of each, and the reciprocals of their lengths. During the time of the electrical communication, the particles of the conducting substance are actuated by a mutual repulsion, which is proportional

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* *As copper conducts electricity with much greater swiftness than lead, perhaps the best mode would be to make the gutters and discharging pipes of that metal. To save ships, ribands of copper should be extended from the masts to the keel.*

† Hence, perhaps, the reason why electricity sometimes vitrifies stones, and calcines metals. There is one experiment, however, that has not hitherto been properly explained: It is, that if the ball of a thermometer be held opposite to an electrified point, the mercury will rise two or three degrees. For the air that streams upon the ball is accumulated, and consequently suffers a condensation, which, according to general analogy, must be attended with a diminution of capacity for heat.

to the difference between the proximate intensities. And the total effort to destroy the texture of the conductor depends partly on its narrowness and slow communicating quality, but much more on its shortness.

ART. II.—*Experiments on the Growth of Pearls, with Observations on their Structure and Colour.* By W. T.

PEARLS, in general, take the colour of the shell in which they are formed, being nothing else than the substance of the shell disposed in concentric layers, and tending more or less to a spherical form. From the great number of small pearls which I have frequently collected from the small sea-mussel *(Mytilus edulis)* so common in the streets of London, I find that there is no part of the flesh of this animal in which pearls do not occur. The natural expenditure of the substance which forms the pearl, is only for the purpose of producing the shell or testaceous covering of the shell-fish; but various causes producing wounds in the animal, or otherwise irritating it, will produce a secretion of the shelly matter to defend the injured part; and however sharp or angular the offending substance may be, it by degrees assumes a round form, in proportion as it is covered by a greater number of coats. The assertion advanced by Linnæus, and repeated in some works, that the Chinese have a mode of producing by art real pearls in the living shell-fish *, though in general little credited, seemed to me so feasible, that I was led to attempt a similar experiment, which I tried upon the large mussel of our ponds *(Anodonta cygnea)*, being the only convenient shell-fish which I could command in the central parts of England. I procured, therefore, the largest of these, from five to six inches long, from the Duke of Marlborough's Water of Blenheim; but although the vigour of the animals promised me

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* Either in Hunter's Museum, or in that of the late Duchess of Portland, was to be seen a pearl-shell, said to be from China, containing a string of pearls, apparently produced in the manner referred to; which is that of introducing iron wires at given distances through the shell of the animal, so as to irritate its flesh, without entering deep enough to kill it.

success, several of them died. I shall only relate what happened in the surviving ones.

I drilled several holes in the most convex part of these shells, and introduced brass-wires of two-thirds of a line in diameter. These wires had a sharp point, and were fixed in the shell in the way of cramps; some were disposed in straight lines, and others in such artificial forms as to shew plainly that the pearls so produced, if the experiment should succeed, were the work of design, and not the unmolested operation of nature. One of these, which, I believe, is still preserved in the Anatomical School at Christ Church in Oxford, is an indisputable proof of this, the points forming the initial letters of my name. I let them down in a wooden box, perforated with holes, and loaded with a weight, into the River Isis; and on examining them a few months after, I found some of them dead, and, it is probable, they died very soon after the operation, as the brass-points remained perfectly naked. I returned the living ones into the water, and took them up at the expiration of eighteen months from the operation. I then killed the animal, in order to examine the shell, and I found, in one instance, the points of the wires fairly covered with a calcareous substance, rather coarser than the inside of the shell, which circumstance was probably owing to the more hasty deposition of the earthy matter, which, therefore, wanted the compactness and the pearly colour of the inner layer of the shell. Instead of points, the wires were now terminated by a round head, and the fish having survived, is a proof that it was sufficiently defended from the injury which it must at first have received. On examining other shells, I found the points of the wires, which projected at least two lines within the shell, covered only with a mucous substance, which, however, preserved a rounded form; so that this mucus, also, resembled the heads of large pins.

In one shell, I found this mucus to be what I then suspected, the nidus for the deposition of the earthy matter, and one or two points observed with a magnifying-glass, shewed the earthy matter deposited in this gluten in the form of round opake white points, which would probably in a longer time have united into one mass, according to the usual process of ossification. This discovery led me to repent of having too hastily destroyed the

animal, as I imagine, that, by fine injection, I should have discovered the vascularity of this gluten, and, probably, inflammation in the contiguous parts. No one doubts the existence of such an animal-nidus in bones and shells, who is acquainted with the process commonly used for exhibiting it, by dissolving their earth in dilute muriatic acid. Shells, in this process, lose their colours, and I find that pearls equally lose theirs, having dissolved blue and brown as well as common pearls, which gave me equally balls, almost without colour, or of a faint yellowish-brown, consisting of concentric membranes, and so light, that from some air-bubbles included, though not visible to the naked eye, they swam in the acid.

These experiments I made many years ago, and, more lately, I commenced others of a similar nature, but better planned, introducing, in place of pointed wires, round beads of different materials, through holes in the shell. These beads were of glass, steel, &c., and, from their form, I supposed would produce less irritation, and afford a fairer surface for the adventitious earthy matter. What I here attempted by art, is not unfrequently produced by nature herself. I have two scallop-shells (the flat-shell), the inner surface of which is very thick, and set with extremely minute points of the substance of the shell, which, it appears, the animal had, in this instance, also produced in its own defence, in order to guard itself against the invasion of a species of boring molluscous animal, which had made its way through the outer layers of the shell, and was proceeding inwards, so as to oblige the animal to secure in many points its inner wall against the invader. These shells seem to point out two facts: 1st, That the animal, being under the necessity of depositing its shelly matter in certain weak points, does not seem to have confined itself to these points only, but, having once begun the process, to have deposited its new matter in many other places. If I am deceived in this conjecture, it is probably on account of the extreme minuteness of many of these points, where the borer had attacked the fish, and which appear to our eyes as depositions without a cause, and made at random. 2dly, That pearls will differ in colour, according as the animal is employed at the time of their formation in producing the coloured or colourless part of its shell. Thus, we have orange-coloured

pearls, as well as silvery ones, produced by the *Pinna marina*, whose shell partakes of these two colours. Thus, also, we have in the small blue mussel of the Scottish seas, and probably of other parts, opaque pearls of a deep blue colour; and I have one of these shells from Montrose in Scotland, in which one of these blue pearls adheres, in the form of an excrescence, to the blue lip of the shell, whilst a dead-white one adheres to the white part of the shell, though without lustre, like the shell itself.

A curious appearance in the flat shells of the scallop, already quoted, seems to confirm the foregoing observation. It is necessary previously to remark, that these shells, when arrived at their full growth, consist of two colours; the middle part of the shell deriving itself from the hinge outwards, is white, whilst all the parts towards the borders of the shell are of an obscure purple or chocolate colour. In this instance, there are not only small purple eminences or pearls rising from the purple part of the shell, but the same purple eminences are profusely scattered over the white part of the area of the shell, and in some places form even a veil of purple, which encroaches much on the white area; whence it seems probable that the fish being advanced to the period of depositing a purple shelly matter, deposited it indiscriminately over all those parts which were then weak, and stood in need of repair.

The blue pearls which are found, though not commonly, at Montrose, are uniformly, as far as I have seen, flattened on one side, where they have been in contact with the shell, which may probably be owing to the largeness of these pearls in proportion to the animal, whereas, the smaller or seed-pearls taken out of the same animal in the London markets are mostly round, and but few of them have a blue tint. Indeed, almost all the Montrose blue pearls are more or less white in some part of their substance. I have one of them four lines in length by two lines high, and as many broad, which is very large in proportion to the size of the animal, and must have been no small encumbrance to it.

The structure of pearls is uniformly radiated from a centre, and consists of concentric coats, when it happens that, in breaking them with a hammer, if they are not split directly through the

centre, a smaller sphere is often seen to occupy the central part. I have broken many such, and have seen this sphere from the size of a pin's head to that of half the bigness of the pearl broken; and as this spherical body or central pearl remains attached to one-half of the pearl so broken, its concave impression is seen in the corresponding half.

Pearls, when broken, are seen to consist of coats of very different colours, and many of those which have a silvery lustre in their outer coat, consist of various shades of brown and yellow within; but I do not recollect that, on the contrary, any pearl externally brown, yielded, on being broken, an internal silvery surface; whence it would appear that the larger the pearl grew, it partook of the silvery coating which forms the internal lining of most shells, notwithstanding that the same pearl may, in the earlier periods of its formation, have contracted some other colour from its local vicinity to some part of the shell of a particular colour. We see also from hence how useless are our endeavours to restore a pearl whose external surface is either decayed or chipped off, as such repairs in those of the finest kind exhibit a discoloured surface, which reminds one of decayed teeth. Many such are to be seen among the Regalia of England, and particularly in one of the Queen's Crowns; and it frequently happens that the coats of pearl are more uneven and rough as we approach the centre, as happens in other calculi, which may have originated in many irregular grains, whether of sand or smaller pearls cemented together. On breaking a pearl of the finest water, of those produced by the river mussel in Scotland, (the *Unio margaritifera*), several of its internal coats were found equal in beauty to its natural one; but, on breaking a fine oriental pearl, it appeared to consist of uniform materials to its centre, whence probably arises the superior lustre and opaline transparency of the latter.

That this uniformity of substance is the real cause of this superior beauty in the finer pearls, is confirmed by the degree of transparency observable in the pearls of the Pinna marina already quoted, which, in some of them, amounts to a half transparency more, and then shewing a darker point in their centre, and occasionally the contrary; but these pearls, like other bodies, the nearer they approach to a uniformity of substance,

are more liable to cracks and flaws than the coarser and more heterogeneous pearls, which are less glassy and more tenacious, as containing apparently more of the animal gluten. Nor is it to be wondered at, that those shells which contain a large proportion of their bulk of colouring matter, should have their pearls for the most part coloured. The pearls of the Pinna marina, when broken, are found to be equally radiated with the others, though their radiated structure is finer in proportion as they are more transparent.

In the British Museum, there is, or was, a famous pink pearl, of a respectable size, and of an oval form: I myself have a small black pearl, perfectly round, and perfectly opake, of which I do not know the history. The pink pearl of the British Museum was probably produced by one of the large West India conchs. I have been told, that, in the waters in Scotland between Perth and Auchtermuchty, the mussels afford green pearls. The blue pearls from Montrose are also radiated in their texture, though extremely dense, so that the radii are invisible to the naked eye.

I noted a particular appearance on breaking a red pearl between two and three lines diameter, namely, that the great transparency of its external coats to the depth of one line, made its central sphere, which was less transparent, appear to the eye, before the pearl was broken, as if a whitish opake sphere had been immediately placed beneath a thin external and very transparent coat; and the same thing I have observed in another red pearl, where the opake central body was extremely small indeed, though perfectly visible, before breaking the pearl, and from the same cause.

The structure of the greater part of shells, if not of all, consists of fibres parallel to each other, but perpendicular to the laminæ of the shell. We see this not only in the recent, but also in the fossil shells, in which latter a suspicion might possibly arise, of their fibrous structure being the consequence of their petrifaction; but their fossil structure corresponding with their natural one, renders such a conjecture superfluous, although it is not to be doubted that the natural texture of animal bodies does frequently give the direction to the sparry matter which afterwards occupies those substances in the earth.

I have further observed, that the external or first-formed coats of several shells, for example the Pinna marina, have the fibres much coarser than the inner or last-formed coats; and I have also observed, in a species of molluscous animal, that the internal shell, which is formed as a shield for the protection of the heart and viscera of that animal, passes from the soft and membranous state to that of a shell, in a manner similar to the ossifications visible in those who have died of hydrocephalus, where many round points or islands of bone extend their edges in every direction, to unite and form a solid crust.

The difference between the fresh-water mussels of different streams in the same neighbourhood is very remarkable. I know that there are different species of mussel inhabiting the same river, for example, the river Severn, at Worcester, in which, I believe, there are at least three species.

In one of the species, the Unio pictorum, I have observed, while the animal was living, very deep erosions on the outside of its shell, and nearly in the most convex part of it. Might a consciousness of these erosions, (that is, of this attack upon its habitation), have given rise to the extraordinary thickness always observable in such diseased shells, or was this appearance merely the result of decay from old age? The shell in such cases easily exfoliated, and these eroded cavities, which were nearly circular, were often lined with a bright green colour, of which I had no opportunity of examining the origin. Another accident to which this species is liable (or at least which I have seen in no other), is the apparent hasty formation of the inner coat of the shell. At the time when I took the animal, almost the whole of the inner surface of the shell was of a livid silvery colour, and this last coat was not in immediate contact with the former coats, but rather resembled a large blister, with an undulated surface. On breaking this inner coat, I found in the cavity a considerable quantity of sand of the river; and, though the two shells of the same animal had not the disease in the same degree, yet, if I found this accident very conspicuous in a shell, I frequently found a tendency to it in the opposite shell, shewing itself in greenish and dusky coloured spots; whence, perhaps, the whole is a disease of constitution, or a weakness of ossifying power, dependent upon old age.

Art. III.—*Notice in regard to Macquarie Island.* By Mr Thomas Raine. Communicated by Sir Thomas Brisbane, Bart., &c.

Macquarie Island is situate in Long. 159° 28′ East, and its north end in Lat. 54° 20′ S. It was discovered by a Colonial vessel belonging to this port, and called by its present name out of respect to the late Governor. It is a long, high, uneven, and mountainous island, extending north and south 30 miles, its breadth being from 2 to 4 miles: it runs higher to the south end than the north. We measured what appeared to us the highest mountain, and found it to be 1750 feet above the level of the sea. Off the north end are two small islets, distant about 8 miles, bearing N. by W. having a clear passage between them and the island. These are called The Judge and Clerk Rocks. At the south end are similar rocks, called The Bishop and Clerk; but these last are about 25 miles distant, bearing about SE. by S., having also a clear passage between them and the island. The soundings on the east side of the island, from a quarter of a mile to 2 miles off-shore, are from 9 to 25 fathoms; after this the bank deepens very fast; the bottom is dark sand, and good holding ground. There are no bays or harbours; hence no shelter, excepting in westerly gales. The coast, though in general rocky, has a few good beaches; some of which consist of coarse black sand, others of large stones. Landing is in general hazardous, owing to the surf; but after a continuance of steady westerly winds, it becomes easy. There is no wood, but abundance of water. The tide rises about 5 feet. No refreshments are afforded by the island. The general form of the surface of the country is mountainous, with conical hills and irregular ridges. On the top of the island are many fresh-water lakes. The largest we saw appeared to be 3 miles in circumference, quite clear, and free from any aquatic plants; the banks either sand or shingle, It is probable that the lakes are supplied by springs within themselves, as there is a constant run of water from them down the mountains. They are said to contain fish, but particularly a kind of trout. Round the margins are many limpets and sea

snail shells, evidently carried thither by birds. Their elevation is from 500 to 1000 feet.

The surface is covered with grass and vegetable mould, so much so that we could not observe the strata. We saw no crater, lava, or any appearance of volcanic origin. Being unacquainted with mineralogy, we are unable to give a description of the rocks; but having picked up a few specimens, we beg your acceptance of them. Amongst them is gypsum, obtained from a large mass on the beach, about 5 miles from the north end of the island, which must have fallen from the hills, as the cliff behind it contained many small loose pieces. There are no mineral or hot springs in the island. There are no four-footed animals. Of birds there are various sorts, viz. penguins, albatrosses, boobies, the sea or Port Egmont hen, shags, wild duck, teal, several kind of petrels, parrots, widgeons, or tussock fowl, a kind of bird that cannot fly, and the mutton-bird. All the small birds, even the parrot, make their nests under ground, so that the declivity of the mountains appears like a rabbit warren. The only domesticated animals are the dogs, taken thither by the sealers for the purpose of catching birds. At one time the island abounded with seals, but now it is a very rare thing to see one; and only four were killed last year. The *Phoca leonina*, sea-elephant, or sea-cow, however, is still plentiful, and alone makes the island valuable in a commercial view; and for the oil obtained from this animal we visited the island. The male sea-elephant is called the Bull, and the female the Clapmatch. They live upon fish, shrimps, and squid, but, when on shore, are never seen to eat any thing. They will remain for weeks in a state almost of torpidity, scarcely moving, if not disturbed, until they are very lean, and so weak as to be scarcely able to reach the water. On their first coming up, the bulls will yield a ton of oil each; but, in general, it requires three bulls, or ten clapmatches, to yield that quantity; the difference being so great between male and female.

The skins have been put to no use hitherto, except for making caps to the men on the island; but it is thought that they will be valuable for making wheel-ropes, coarse thongs, coach-harness, and glue. The blubber lies immediately under the skin, is about three inches thick on the clapmatches, and from two

to six on the bulls. They have no fur; the hair is flattish, and resembles that on the tail of the platypus; varying in colour from a light to a dusky brown. The cubs are at first black, with soft hair; but they soon lose this for the brown strong hair already mentioned. A young cub is about three feet long; a clapmatch is from 10 to 12 feet long, and a bull from 15 to 20. The clapmatches come up to pup about the latter end of September, and remain till the latter end of November; they form rookeries or herds, consisting of from 20 to 70. The bull stations himself between them and the water, and if any clapmatch moves off, he forces her back into the rookery, and will even go out to sea after her. They have never been observed to have more than one cub at a time, for which they show great affection: if disturbed, the clapmatch will leave it with tears trickling down her face, and hobble into the water, the bull following as just mentioned. Their motion is of a vermicular, or writhing kind, but is greatly assisted by the two flippers at their shoulders; they make no use of their tail, but seem to drag it after them. The teats are situate at the lower part of the belly towards the tail, and are two in number. There are no mammæ externally, nor does any nipple project; the milk is yellowish, and thick. Almost immediately after parturition the bull has connexion with them, and hence it is concluded they go twelve months with young. The cubs make a noise, barking like a dog; the clapmatch makes a kind of howling, but the bull varies his voice according to circumstances.

On his coming out of the water, he looks round for a rookery, and sounds his trumpet, (as the sealers term it, a noise very much resembling the blowing of a conch-shell); but should he come up at a rookery, and a bull there, the challenge is accepted, or one retreats. Two bulls are seldom seen at one rookery; if two old bulls meet, they have a pitched battle; one becomes victor, and joins the clapmatches, and is called the Beach Master. When you attack a bull, or come close to him, he raises himself on his flippers, and gapes, making a noise through his throat like that of a person gurgling; if possible, he makes for the water, always facing you, and moving backwards. About the end of December, a large kind of clapmatches come up, which are called Brown Cows; it is uncertain whether they are the same

which lately pupped, or different altogether. The latter is the prevailing opinion; and they are considered barren, as they are never known to pup. Young bulls come up at the same time, and are called Christmas Bulls. These leave the island the latter end of January. The next season is March, when a few bulls come up, and are called the March Bulls. As the sea-elephant has never been observed at any considerable distance from land, it is the opinion of the sealers that they lie at the root of the island, and it is imagined that they take in ballast for this purpose, as they are observed on coming up to vomit a quantity of pebbles.

Few whales have been seen at the island, and these were the black or proper whale. The fish called by whalers the Killer, is very common: it is said to kill its prey, by goring it in the belly with its dorsal fin, which is sharp, and sometimes from 8 to 10 feet long, shaped like a scythe, and frequently appearing above water. There has been no fish caught at the island by any of the vessels that have visited it; one of our gang saw one like the guard-fish. We saw none of the crustacea, excepting shrimps. Skeletons of the sea-louse and craw-fish were found, however, on the beach. The limpet and sea-snail are common. No corals, echini, or mollusca, were seen; and no sponges.

No insects appeared, excepting a small fly, found under the stones about the beaches. As summer advances, probably others might appear.

No tree or shrub exists on the island. Amongst the herbaceous plants we observed a singular nondescript species of *Tussilago*, furnished with ovato-lanceolate leaves, covered with a dense silvery pubescence, with solitary purple flowers; an umbelliferous plant, with palmate leaves, the petioles and both surfaces of the leaves hirsute, the umbel simple; a small species of *Sipirus;* a species of *Vaccinium;* and a species of *Ranunculus;* a beautiful fern; several mosses, some of which appear rare, if not new; and a species of *Carex*, called Tussock, with which the island is nearly covered.

The thermometer, during our stay, in October and November, ranged from 30° to 44°, rising or falling according as the wind

blew from the north or south. On shore, the thermometer generally stood 6° or 7° higher than on board. Our barometer unfortunately having been broken, we are unable to say any thing respecting its changes.

The prevailing winds are from the NW., the W., and SW., easterly winds seldom blowing. During our stay we experienced three easterly gales, two of which blew very strong. We occasionally had snow-squalls and hail, but no thunder or lightning. The snow never remained above two days on the ground.

We saw no ice on the island, excepting icicles at the rivulets. About three years ago an ice-island was seen, and so large as to ground in 80 fathoms water. We twice observed a brightness to the southward, in the form of steady rays, which we supposed to be the aurora australis. The falling stars were particularly bright, and so were the constellations. The phosphoric appearance in the sea-water is very bright.

The gangs employed in fishing live in huts constructed with rafters, thatched with the tussock, and sometimes covered with the hides of the elephants: their fire is the blubber. Exclusive of the salt provisions left with them, they eat the hearts and livers of penguins, the eggs of birds, particularly those of the penguin; young albatrosses and mutton-birds, which they salt, and of which they have always a store thus prepared; widgeons, parrots, teal, and elephants' tongues.

In enumerating the animals, I omitted to mention the sea-leopard, so called from the skin being spotted. It is said to devour penguins, young pups of the sea-elephants, and has been known even to attack a boat. There have been six varieties of penguins seen, four of which are common, and regularly visit the island; the other two have only been seen once, and that last year. Drift-wood is often found on the beach, which is known to come from Campbell's Island, that island having much of the sort growing upon it. On the west side there is also a part of a topmast of a ship of 460 or 500 tons; together with a studding sail-boom; and the ribs and vertebræ of a whale.

SYDNEY,
February 4. 1822.

ART. IV.—*On the Formation of Dew.* By HENRY HOME BLACKADDER, Esq., Staff Assistant-Surgeon.

SIR,

HAVING for many years had my attention directed to subjects connected with meteorology, and having been led first to doubt the soundness of certain opinions, either generally received or confidently advanced, on that subject, and ultimately to form others of an opposite nature, I am induced to request your attention to a few general remarks on the spontaneous evaporation and condensation of moisture. It is impossible that, in the compass of a letter, such a subject can be noticed otherwise than in a very cursory manner. I have therefore to make the additional request, that you will consider what I may at present advance, merely as a few extracts from numerous notes on meteorological subjects, and not arranged, perhaps, with all the attention that might be wished.

You are aware, that, at present, the general opinion is, that the decrement of heat, which is observed to precede the formation of dew, and which takes place, on certain occasions, at the surface of snow, &c., cannot be accounted for on the principle of evaporation; or, in other words, that the diminished temperature observed on such occasions, cannot be the consequence of that absorption of heat which takes place during the conversion of moisture into the state of an elastic fluid or vapour. You are also aware, that the depression of temperature in question has by some been attributed to the radiation of heat into regions of empty space,—and, by others, to "cold pulses showered down from the atmosphere." By the respective supporters of each of these hypotheses, the influence of evaporation seems to be altogether excluded: and is not this circumstance alone sufficient at least to place one on his guard?

In the course of the following remarks, reference has been made to facts, which, it is hoped, have been well ascertained; and on these chiefly the reasoning is founded. Those who may be disposed to call these facts in question, have almost constant opportunities of either verifying or disproving them. A more minute detail, therefore, seems at present to be unnecessary,

even had the limits to which I have restricted myself otherwise admitted of it.

It has been ascertained, that, on a serene evening, after sunset, when dew is about to form, the air is never in a state of tension or saturation; and that, even when a visible condensation of moisture has taken place on the blades of grass, the air is still capable of receiving an accession of moisture. As dry air, therefore, or air not saturated, cannot remain in contact with a moist surface without the existence of evaporation, whatever may be the respective temperatures of these bodies, it follows as a necessary consequence, that evaporation must be going forward, from a grassy surface, during the formation of dew; and it might be inferred, not without some degree of probability, that these phenomena were not altogether disconnected. During the process of evaporation a quantity of heat is always absorbed, or becomes insensible; and if there is not a constant influx of heat equal to that which is thus absorbed, the temperature of the body from whose surface the vapour is generated, is always very sensibly lowered. We find, accordingly, that on a hot summer-day, when the evaporation from a meadow is very copious, the temperature of the grass is not reduced below that of the air, as the heat absorbed in the vaporific process is constantly supplied by the sun's rays. But as the sun declines, the influx of heat diminishes, and at sunset may be considered as altogether suspended. Still, however, the air not being saturated, the process of evaporation goes forward, and, as the supply of heat is now cut off, we would be led to expect that the temperature of bodies capable of supporting evaporation would suffer a sensible depression. This is also agreeable to observation; for even before sunset, the temperature of the grass is often sensibly diminished, and, in the course of the succeeding hour, generally indicates a very remarkable decrement of heat. When, therefore, we find that the degree of cold that takes place on a grassy surface during a calm state of the air, always bears some relation to the dryness of the latter body, the most natural conclusion would certainly be, that evaporation is the means by which the diminished temperature is produced.

It has been observed, that different bodies, equally fitted to support evaporation, and similarly exposed to the influence of

the air after sunset, have their temperatures very differently affected. Thus the soil of a garden is less cooled than a gravel-walk, and the latter much less cooled than a grassy surface; and these facts admit of the most satisfactory explanation, by a consideration of the respective facilities with which these bodies acquire supplies of heat by conduction from the ground. Garden-mould is a much better conductor than gravel, and the latter is greatly superior to grass and other vegetable bodies; hence the temperature of the latter always suffers the greatest depression. There is, however, one circumstance connected with grass and other vegetable bodies which requires to be adverted to, and which, if it could not be satisfactorily explained, would render the influence of evaporation on such occasions problematical It has been observed, that the temperature of a grassy surface is often diminished, and sometimes remarkably reduced, while the surface of the blades of grass, &c. seemed to be destitute of moisture, and hence, to appearance, little fitted to support evaporation. But this apparent difficulty vanishes, when we consider that vegetables have an organization which enables them to transpire moisture and various gaseous bodies, and that similar to what takes place with the insensible perspiration of animals, without necessarily producing any apparent wetness of their surfaces.

Having thus attempted to show that evaporation is the means by which the temperature of grass suffers a depression on a serene evening after sunset, it remains to be explained how it afterwards comes to be dewed, and even at a period when the air is not saturated with moisture. It has been found that, on dewy evenings, the ground under a grassy surface is always warmer than the air, and consequently considerably warmer than the cold grass. As, therefore, the soil under the grass is moist, and in contact with the air, evaporation must be going forward at its surface; and the vapour that is there generated must, as it rises upwards, pass through or between the blades of grass. We might therefore expect that part of this vapour would be liable to be condensed by coming into contact with the cold grass, in its progress upwards. We might also be led to expect that this condensation would take place at an earlier or a later period, according to the state of the air, in respect to its greater or less

disposition for moisture, in some degree modified perhaps by the relative temperature of the ground, and the facility with which it can furnish a supply of moisture. This also is consonant with observation; for, when about sunset the air is still capable of receiving a considerable accession of vapour, the moisture transpired by the grass is rapidly carried off, and likewise that at the surface of the subjacent soil. Hence in such circumstances the temperature of the grass is considerably reduced, without any manifest deposition of moisture on its surface. When, however, the temperature of the grass has suffered a certain reduction below that of the vapour issuing from beneath it, and when the air, from its decreasing temperature and the effects of evaporation, has its disposition for moisture diminished, the vapour generated at the surface of the subjacent soil is partly condensed by coming into contact with the piles of grass, and, sooner or later, according to circumstances, acquires the form of distinct drops of dew.

It perhaps cannot be easily shewn, that a solid body has its temperature always as much increased from the condensation of a certain quantity of vapour on its surface, as it would in similar circumstances have its temperature diminished by the evaporation of a like quantity of moisture. Yet as heat is always extricated on the condensation of vapour, it cannot be questioned that grass must have its temperature in some degree affected when dew is deposited on its surface; and it has been found, that though, on certain occasions, when the condensation is sudden and copious, there is an evident accession of heat, it frequently happens that the previous degree of cold is not sensibly diminished. If, therefore, it were found that the air is completely saturated whenever dew begins to form, and if it were the case that grass readily acquired heat by conduction from the ground, this circumstance of grass maintaining its depressed temperature after moisture has been condensed on it, could not be satisfactorily explained on the principle of evaporation. But we learn by observation, that the state of things is exactly the reverse of what we have supposed. For, dew may often be observed on the grass long before evaporation has altogether ceased; and, as has already been observed, grass acquires but little heat by conduction from the subjacent soil. As long,

therefore, as the heat acquired from the condensation of moisture is not greater than the measure contemporaneously absorbed in the process of evaporation, the temperature of the grass will not be sensibly increased. But it now becomes necessary to remark, that, as the grass becomes colder from the influence of evaporation, the contiguous air is at the same time cooled by communicating its heat to the grass; and were it not for this influx of heat from the air, the degree of cold produced by evaporation would be much greater, and would be continually increasing as long as that process continued in action. But as things are constituted, the cold produced by evaporation only increases until the contiguous air can furnish a supply of heat equal to that which is absorbed in the formation of vapour; and hence, in the ordinary course of nature, the depression of temperature soon attains a certain limit, beyond which it cannot extend, unless we suppose the capacity of the air for moisture to be somehow increased without a contemporaneous elevation of its temperature.

As the air, by losing heat, becomes denser, and consequently has its specific gravity increased, that which has been most cooled by contact with the grass must retain the lowest position, and hence will remain contiguous to the grassy surface, provided there be no lower level to which it can remove. We find, accordingly, that over a level meadow, whose surface has been cooled by evaporation after sunset, the air increases in temperature as we ascend upwards; and that the air in contact with the grass has always the same, or very nearly the same, temperature with that body. If, then, the atmosphere be calm, and there be no lower level to which the cold air can withdraw, the grass remains immersed in a body of air as cold, or almost as cold, as itself; for that which is higher and warmer cannot descend, so as to come into contact with the grass; and the quantity of heat that can be conveyed by conduction through air is well known to be extremely small, and on the present occasion need not be estimated. If the temperature of the contiguous air, therefore, diminishes with that of the grass, and if the latter, after having been cooled by evaporation, should acquire an accession of heat, from a condensation of vapours rising through it from the subjacent earthy surface, this acquired heat would be partly ab-

stracted by the contiguous air; and this, by having its levity increased by an elevation of temperature, would necessarily recede upwards, thereby leaving the grass still in contact with air of a low temperature. Hence it follows, that, partly by the process of evaporation, and partly by the recession of the contiguous air, the depressed temperature of the grass may be maintained, at a time when moisture in the form of visible particles is condensed on its surface.

In considering the causes of that condensation of aqueous vapour which takes place in certain states of the atmosphere after sunset, our attention has hitherto been confined to what may be termed, for the sake of distinction, the Primary Formation of Dew; that is, a condensation of vapour produced by moist air, or vapour, coming into contact with a solid body of a lower temperature, and at a time when the contiguous air is not otherwise in a state of saturation. And, in order to simplify the investigation as much as possible, the phenomena which accompany the formation of dew on a grassy surface, have hitherto almost exclusively occupied our attention. That which may be termed the Secondary Formation of Dew, and which is usually by far the most copious, remains to be investigated. By the secondary formation of dew is intended a deposition of aqueous particles from the air, from its capacity for moisture being diminished by a depression of its temperature, and that not immediately produced by its coming into contact with a solid body of a lower temperature.

On a serene evening, favourable to the formation of dew, the atmosphere is not only calm, but often clear, and free, or almost free of clouds; and as long as this state of the air continues, we may conclude that it is not, at least in ordinary cases, in a state of complete saturation. But we have frequent opportunities of remarking, that though the whole atmosphere be obscured by a fog, so that objects cannot be distinctly seen at a short distance, and though it remain for days in succession thus loaded with moisture, still it is not in a state of saturation; on the contrary, evaporation is even then so active as to produce a very considerable depression of temperature in bodies having a wet surface. It is also certain, from observations made in almost all climates, that wherever the surface of the earth is capable of supporting

evaporation, the lower air is never free of aqueous particles, such as constitute a mist or fog. In all countries, the surface of the earth, when viewed at a distance, appears enveloped in a haze, of greater or less density; and though, when sailing, for example, on the Pacific Ocean, the atmosphere appears very clear, and the sky brilliant, it is found, on being viewed from a high mountain, that the former is constantly obscured by a haze of considerable density, and which extends many feet above the surface. During the formation of dew, when the general atmosphere appears clear, and objects at a moderate distance seem as if surrounded by a perfectly transparent air, aqueous particles, such as constitute a haze, are never altogether absent; the quantity of these particles, and the greater or less rapidity of their formation depending on the particular state of the air in regard to moisture, as influenced by temperature. When they are in such quantity as to produce an indefinite obscuration of the air near the surface, they constitute a *haze.* When the obscuration is more complete, with a defined outline, from the particles congregating in the form of a cloud, and either resting on the surface, or elevated a few feet above it, it is termed a *mist.* And when the whole atmosphere appears equally obscured, it is called a *fog.* This last is sometimes seen shortly after sunset, but most commonly makes its appearance towards morning; and has on some occasions been ascertained to extend to the height of from 50 to 100 feet above the ground; its upper surface being horizontal, well defined, and having the appearance of a dense white cloud. This fog, however, must be distinguished from that obscuration of the air which frequently, and at different seasons, accompanies easterly and westerly winds, and which would require a separate investigation.

It has already been observed, that both before and after dew has been deposited on a grassy surface, evaporation goes forward not only at the surface of the blades of grass, but also from that of the subjacent soil; and it was also observed, that though the temperature of the latter gradually diminishes after sunset, it always remains higher than that of the air at some distance above the ground, and consequently must be considerably above that of the grass, and of the air in contact with it. As heat promotes, it follows that a loss of heat will retard evaporation; more espe-

cially when the supply of moisture, upon which that process depends, is furnished by the peculiar action of an organised body, the regular exercise of whose functions is greatly influenced by the presence or absence of a certain degree of heat. When the temperature of the grass is greatly, and, as often happens, suddenly reduced, we may expect, from what is known to take place in similar cases, that its transpiration will be diminished, though not altogether suspended, and, consequently, that the evaporation from its surface will be less copious.

On the other hand, as the temperature of the soil under the grass diminishes very slowly, owing to the facility with which it acquires heat by conduction, the evaporation from its surface must continue comparatively active, long after that from the grass has become almost extinct; and even should the air become saturated with moisture, vapour will continue to be generated at the earthy surface, until such time as the temperature of the latter comes to be in equilibrium with that of the contiguous air.

When, therefore, the disposition of the air for receiving moisture, and the temperature of the grass, are such as to admit of the vapour generated at the surface of the subjacent soil being partly condensed into dew, as it comes into contact with the piles of grass, part of this vapour, as it rises through the cold air, will also be condensed into aqueous particles, and remain suspended in the air. At first, and on some occasions, these particles are so thinly dispersed as to be almost invisible; but afterwards, and on other occasions, their numbers increase with such rapidity, as very speedily to produce a general haziness over the surface. When this occurs early or soon after sunset, and when the particles congregate, so as to form a mist, the lower surface of which is elevated from 15 to 30 feet above the ground, those who inhabit parts of the country, in the vicinity of extensive meadows, consider it as an indication of approaching rain; and they express it by saying, "The dew is rising, we shall soon have rain." In this instance, as in many others of a similar nature, the general observation is founded in truth, while the principle upon which it is grounded is manifestly erroneous. The appearance referred to occurs only after a continuance of hot weather, with the prevalence of southerly winds; and at a

time when the quantity of moisture, united with the air, is very considerable. The soil under the grass is also more than usually heated, and hence, after sunset, the air immediately in contact with it, and the vapour generated at its surface, are elevated to some distance above the ground, previous to their being so cooled as to produce a condensation in the form of a mist. It may be considered as a strong proof that this mist is condensed vapour rising immediately from the ground, that the space intermediate between its lower surface and the earth is always obscured, more or less, by a haze; and that while its upper surface is always marked by a well-defined line, its under surface is much less distinct, so that it is difficult to determine its exact boundary; or rather we should say that it gradually dies away into a dilute haze, scarcely perceptible as it approaches the surface of the ground. As the night advances this mist subsides to the surface, and is ultimately deposited on the grass, constituting part of what has been termed the Secondary Formation of Dew. Those who account for the origin of such mists, by supposing them to be produced by the subsiding of aqueous particles floating in the air, whether in the form of clouds or otherwise, will find some difficulty in satisfactorily explaining how they usually make their appearance over the tracks of rivers, canals and ditches, over collections of stagnant water, and over meadows, or parts of meadows, where draining has either been neglected, or has proved only partially successful. But, though the opinion referred to be incorrect, it is at the same time certain, that on many occasions the aqueous particles suspended in the air during the day, are observed to subside towards evening, and in the course of the night; and, at the same period, the particles congregated in the form of clouds, are often observed to separate and disperse themselves through the air, in the form of a haze, which renders the azure sky obviously less brilliant, though the air still retains a considerable degree of transparency.

As the air becomes more and more expanded as it increases in temperature during the day, so in like manner, after sunset, it becomes more and more contracted as it becomes colder, from the supply of heat being cut off, and from the refrigerating influence of evaporation at the surface of the earth. The contrac-

tion produced by the decreasing temperature of the lower air must produce a subsidence, especially in calm weather, of the superior body of air; and if a cloud be suspended in the latter, the same appearance will be produced as if the cloud approached nearer the earth by descending through the air. But as long as a cloud has the same temperature with its suspending medium, and is not operated upon by contending or modified currents of air, it never approaches nearer to the earth, excepting in connection with the body of air in which it is suspended.

It is a common opinion, that clouds at a distance from the earth are *dissolved*, sooner or later, when the body of air in which they are suspended is not saturated with moisture; but it has never been explained how clouds are so commonly dissolved in the evening, when the air, by becoming colder and more dense, must be less disposed to receive an accession of moisture in the form of vapour. It is evident, indeed, that the mere *dispersion* of a cloud has been mistaken for its solution, or its union with air in the form of a pellucid vapour. The distinction, however, is both obvious and necessary. There is reason to believe that it is a rare occurrence for the air in the free atmosphere, to be in a state of perfect saturation; and perhaps we have as yet no instrument that can exactly, and in all circumstances, indicate that state. It is pretty evident, however, that the particles of moisture congregated in the form of a cloud, very soon begin to separate, when the air in which they are suspended attains, and probably when it nearly approaches, to a state of saturation. It is possible, that, on such occasions, the cloud may acquire such a density, and the air may attain to a state of saturation with such a rapidity, that a deposition of rain may be produced. But it happens much more frequently, that the particles constituting the cloud are gradually dispersed through the air, in the form of a haze, and subside towards the surface of the earth; and sometimes, especially towards morning, obscure the whole atmosphere in the form termed a fog; while part is deposited on the grass, and other bodies on the surface of the earth; thus greatly increasing the secondary formation of dew, and damping all bodies indiscriminately that are freely exposed to the air.

But, have we reason to believe that air, not saturated with

moisture, is capable of dissolving the particles of a cloud that may be suspended in it, if they are both of the same temperature, and if there be no extraneous supply of heat? When the rays of the sun impinge on a cloud, it is possible that its particles, or some of them, may be enabled to assume the form of vapour; but if, during the sun's absence, clouds are dissolved by the air, a very remarkable degree of cold must be produced; for it must take as much heat to convert a particle of water into vapour when it is suspended in the air, as in any other situation.

When air, capable of absorbing moisture, is in contact with a moist surface of the same temperature, and which has no means of acquiring heat, except from the air in contact with it, a particle of water cannot be converted into vapour without a portion of heat being abstracted from the solid body, and whose temperature is thereby always sensibly reduced. Neither the air nor the particle of moisture could, therefore, supply the requisite measure of heat, to convert the latter into vapour. If, then, a particle of moisture on the surface of a solid body cannot be converted into vapour, without heat being abstracted from the solid body, how is an aqueous particle suspended in the air to be dissolved?—from whence is the additional measure of heat to be derived? If, again, dry air can convert the aqueous particles suspended in it, and having the same temperature, into vapour, how is it that evaporation is often very active, from the surface of wet solid bodies, at a time when the whole atmosphere is obscured by a thick fog; and that whether the moist body be in the shade or otherwise, whether the sun be below or above the horizon? It certainly would be the most obvious conclusion, that, in the absence of the sun's rays, and as long as there is no extraneous supply of heat, aqueous particles, when suspended in the air, cannot assume the form of a pellucid vapour.

As the cold produced by the refrigerating influence of evaporation is the cause of the primary formation of dew, a greater intensity of cold by the same means, causes that condensation and crystallisation of vapour termed *hoar-frost;* for this is only a particular form of dew, and differs from it only inasmuch as

snow is different from rain. We learn from observation, that evaporation always accompanies the formation of hoar-frost; and when dew is forming on low plains, hoar-frost is often formed at the same instant, and at a small distance, on parts of the earth's surface having a general elevation of a few hundred feet.

Next to the formation of hoar-frost, that reduction of temperature which has been observed to take place at the surface of snow, may perhaps be considered as most closely allied to the cold that takes place previous to the formation of dew. On a clear night, the surface of snow has been observed to be even as much as 16° below that of the air, and this degree of cold extended but a small way below the surface. So great a difference between the temperatures of the surface of snow and of the air is not of frequent occurrence; but a difference of several degrees is far from being a rare phenomenon; and whenever it occurred, the air was never in a state of saturation,—evaporation was always found to be going forward. If, therefore, it be well known that evaporation is capable of producing very intense degrees of cold, and if we find a solid body whose surface is capable of supporting evaporation, greatly reduced in temperature, at a time when that process is going forward at its surface, are we not entitled to conclude that they are in the relation of cause and effect? And, would it not be highly unphilosophical, in such a case, to admit the existence of other hypothetical causes, even should they merit the title of ingenious or plausible?—though, by the way, a much more applicable term might readily be selected.

A theoretical objection, indeed, has been made to the fact of evaporation being the cause of the reduction of temperature observed at the surface of snow *. Reference is made to the observations and experiments made at Glasgow by Dr Wilson, in the year 1780, as detailed in the London Philosophical Transactions. That gentleman found the temperature of the surface of snow on a clear night to be 23° below zero. The tempera-

* See *Suppl.* to the *Encyclop. Brit.* vol. iii. p. 555.

ture of the air a few inches from the surface was from 8° to 10°, and at the height of 24 feet about 14° higher than that of the snow. "This excessive cold," we are told, "was evidently not occasioned by evaporation, for, on blowing with bellows against the bulb when it lay on the snow, so far from sinking more, the mercury actually rose two degrees higher than its station in the air." If the gentleman who starts this objection give the subject a little farther consideration, he will find that the evidence is not so clear as he imagines. In the first place, the air which entered at the valve of the bellows was doubtless 8° or 10°, more or less, warmer than the snowy surface. In the second place, if he will take the trouble of repeating the experiment on a fitting occasion, and take the precaution of, having a thermometer inserted into the bellows, through a perforation made in the upper board, near the muzzle, he will find no difficulty in discovering from whence the additional measure of heat was derived; nor any thing remarkable in the circumstance that a thermometer should indicate an increase of temperature, at a time when a current of relatively warm air is directed against its bulb.

Not only, however, is evaporation the immediate cause of the cold which determines the formation of dew and hoar-frost, and which reduces the temperature of the surface of snow, but is also *frequently the chief agent* in that absorption of heat which induces congelation; whether that be exhibited in the freezing of the earthy mould, or in the solidification of water. In this country, congelation never goes forward at the surface of the earth when the air is in a state of saturation.

It would be a curious, though not very useful investigation, to endeavour to estimate how much the temperature of the air is influenced by evaporation, or, what the temperature of the earth and atmosphere would be, if that process had no existence in nature. Air having its density increased, by parting with a portion of its heat at the surface of the earth, cannot easily acquire a greater elevation. Currents of wind may have some influence, and, in a calm state of the air, the internal commotion excited by the vaporific process has an evident effect. On such nights, when the refrigerating influence of evaporation is most operative, the temperature of the air at the height of 300 feet is

probably but seldom, if at all, affected by that of the surface; and at that height, it is found, on ordinary occasions, to be about one degree lower than at the level of the sea. But, we cannot infer that the temperature of the air at that height, or at a height beyond the cooling influence of the ground, would be the same on a calm evening, if no evaporation existed. Though the vaporific process may not reduce the temperature of the earth's surface during the day, it is the means of preventing an immense accumulation of heat. Thus, in the deserts of Arabia, where evaporation is comparatively null, the temperature of the air in the shade has been observed as high as 120°; and even in this country, and at sea, the deck of a ship often acquires a very intense degree of heat. What, then, might we expect the temperature of the earth and atmosphere to be, if evaporation nowhere and at no time existed!

Had the preceding remarks been less extended, it might now be shewn, that all the objections hitherto made to the influence of evaporation in producing that decrement of heat so commonly observed in connection with the formation of dew, and on like occasions, can be readily and completely obviated. The peculiar phenomena that have been observed upon glass, metals, and other substances, when similarly exposed; and the different results that are derived from a difference in the position, &c. of the same substance, may all be satisfactorily explained on the same principle: that is, that the absorption of heat which takes place in the conversion of moisture into the form of vapour, is the means by which the depressed temperature in question is brought about.

The explanation of these, however, and other interesting points, will be given at a more convenient opportunity, when the whole subject may be brought under more particular consideration.—I am, &c.

To Professor Jameson,
21. Royal Circus.

Art. V.—*A popular View of Mr Barlow's Magnetical Experiments and Discoveries, particularly as they have been rendered applicable to the Correction of the Local Attraction of Vessels.*

Several detached notices of Mr Barlow's experiments and results, have been given in some of the preceding volumes of this Journal; but the great importance of them to nautical science, has been lately so strikingly demonstrated by a Report of the Author's, addressed to the Admiralty, detailing a series of experiments made, by order of that Board, in his Majesty's vessels in various parts of the globe, that they now possess a new and more general interest; and we feel assured, that a connected and popular view of the whole subject will be acceptable to our readers in every part of Europe.

Of the numerous interesting facts with which philosophy has been from time to time enriched, by far the greater number may be traced to some fortuitous or accidental circumstance; and it belongs perhaps almost exclusively to the nineteenth century, to boast of some valuable discoveries, which, independent of chance, have resulted from scientific investigations and experiments directed to a specific object: Of these, the safety-lamp of Sir H. Davy, and his present chemico-electric guard to the copper of vessels, and the correcting plate of Professor Barlow, form memorable examples; and in all these cases, the value of the discovery is only equalled by the extreme simplicity of the application.

It is now at least 600 years since the compass began to be employed as a nautical instrument; and yet it is only within a short period, that an imperfection has been discovered in it, which detracts much from its real value for such a purpose, namely, that the needle does not continue to point in the same direction, with the ship's head at different points, the difference in some cases being so great as to lead to the most fearful errors and uncertainties. The general nature of this effect will be understood, by considering, that the upper parts of all iron bodies attract that end of the needle, which, when freely suspended, dips below the horizon, that is, the north end in the northern, and the south

end in the southern, hemisphere. Now, as in vessels of all kinds, the iron which enters into their construction or equipment is nearly symmetrically disposed with respect to the axis, or longitudinal section, of the ship, when this axis is in the magnetic meridian of any place, the action of the iron is either coincident with, or directly opposed to, the magnetic action of the earth, and in either case the lateral direction of the needle is not disturbed: but when the ship's head is on any other point, and particularly towards the east or west, the two forces acting on the needle being nearly at right-angles to each other, the latter is drawn more or less out of its natural position, according to the direction of the resultant of these two forces; and the consequence of this is, that the course shown on shipboard by the compass, differs from the actual course of the vessel, by a quantity equal to the whole of this disturbing force, and which, in some cases, as we shall see, amounts to two or three points of the compass, that is, to 20°, 30° or 40°, but varying with every position of the ship's head, and with every change of terrestrial situation, from one pole of the earth to the other, and according to laws which, till the present time, seemed to bid defiance to every attempt made to unravel them. We have said, that it is only within a short period that this disturbing power has been observed; but it should be stated, that some obscure notices of such an effect are slightly alluded to by navigators of anterior date, as for example by Dampier, by Cook, and one or two French navigators. It does not however appear, that, in these instances, any thing more than the mere fact is stated, without any idea being thrown out respecting the cause, and much less of any remedy for the anomaly in question. The cause is, we believe, first distinctly noticed by Mr Downie, Master of His Majesty's ship Glory, in his report to the Admiralty, published in Walker's Treatise on Magnetism, in the year 1794 This experienced officer says, "I am convinced that the quantity and vicinity of iron in most ships, has an effect in attracting the needle; for it is found by experience, that it will not point in the same direction when placed in different parts of a ship; also it is rarely found that two ships steering the same course by their respective compasses, will go exactly parallel to each

other; yet these compasses, when compared on board the same ship, will agree exactly."

A few years after this, the action of the iron of the vessel was more minutely noticed by Captain Flinders, who was the first to trace its connection with the dip of the needle, and to point out that the effect was different in quality on the contrary sides of the magnetic equator, and increasing in quantity as the dip in either hemisphere increased; and by him the subject was brought under the notice of the Admiralty Board, who ordered experiments to be made in different ships, in order to ascertain the general amount of the error thus produced. The inquiry was, however, again lost sight of, till Mr Bain published his valuable treatise on the "Variations of the Compass," in which the fatal consequences attending this source of error, are put in so clear a point of view, as to strike the most indifferent and uninformed readers; and whatever advances have since been made towards correction, the nautical profession will owe much of it to observations contained in this useful work. It happened, that, at this time, our arctic expeditions were in contemplation; and the local attraction of the vessels in those seas, was one of the objects to which the attention of the officers was particularly directed. The results of the experiments made in these instances, are given by Captains Ross and Parry, in the accounts of their respective voyages; and the amount of the disturbing force was found to be such, as to call for some prompt and efficient remedy, the difference of the bearing of an object having been found by Captain Sabine to be at least 50°, merely from a change of position of the ship's head from east to west.

When the great amount of this error is thus pointed out, it will seem extraordinary that it should have remained so long unnoticed. It may, therefore, be proper to observe, in justice to the memory of the many excellent navigators, whose names and discoveries now only live in history, that this effect was much less formerly than it is at present, in proportion as the quantity of iron used in the construction and equipment of vessels was less than at this time. It is only within a few years that pig-iron has been employed for ballast, the weight of which, in some vessels, exceeds *three hundred tons;* an immense surface of iron is also introduced, by the admirable invention of iron-tanks, to

supply the place of the old water-casks. Moreover, the knees, sleepers, and, in some cases, even the riders are now of iron: hempen cables have been supplanted by those of iron; and some attempt has been recently made to employ gun-carriages of the same material. But of all innovations of this kind, the patent capstan by Captain Phillips, a highly valuable construction, has, perhaps, from its form and situation, the greatest effect on the compass; indeed, its action is so powerful, as will be seen as we proceed, that, without the means afforded by Mr Barlow's correcting plate, it must of necessity have been prohibited in all vessels of a smaller class than frigates. In the Griper, for example, the local attraction was 14° at east and west, making an extreme difference in the river Thames of 28°, which was reduced to about 16° by the removal of the capstan.

Having thus made our readers acquainted with the nature of the errors which nautical men had presented for philosophical investigation, let us follow Mr Barlow in the experiments he undertook, with a view to discovering some means of correction. It should be observed, that, at the time of which we are speaking, little or nothing was known of the mathematical laws of magnetic attraction. It had been ascertained, that, while a compass-needle was placed near the upper end of a bar of iron, the north end was drawn towards the bar, and that, near the bottom, the south end of the needle was attracted; and it consequently followed, that there must be some intermediate point, in which the effect of both ends was neutralized. It was also known, that a large mass of iron attracted more powerfully than a smaller mass; and that the effect was greater, as the distance between the iron and compass was less: and amongst other ratios it had been stated, that the power varied inversely as the cube of the distance; but still, no explicit and connected laws had been established: this, therefore, was the first object of the author. With this view, he procured a solid iron-ball, thirteen inches in diameter; and placing his compass above this, he found, as in the case of the bar, that the north end of the needle was attracted by the ball; that, when it was placed below it, the south end was attracted; and that, by causing his needle to descend in any vertical, it always passed through a point where the iron had no effect upon it. The question then occurred, Are all

Fig. 2.

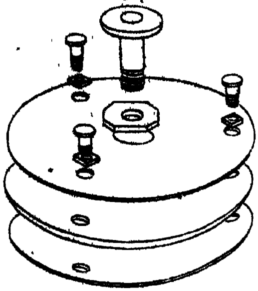


Fig. 3.

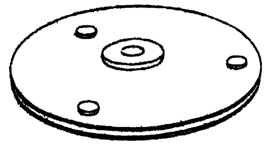


Fig. 6

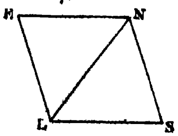


Fig. 7.

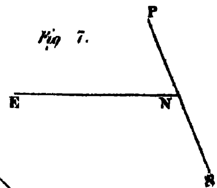


Fig. 1.

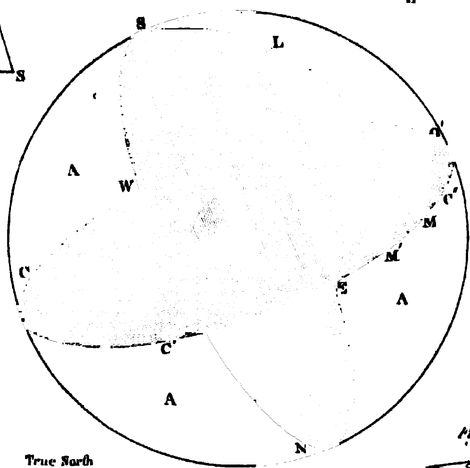


Fig. 4.

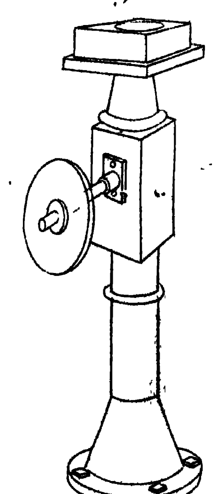
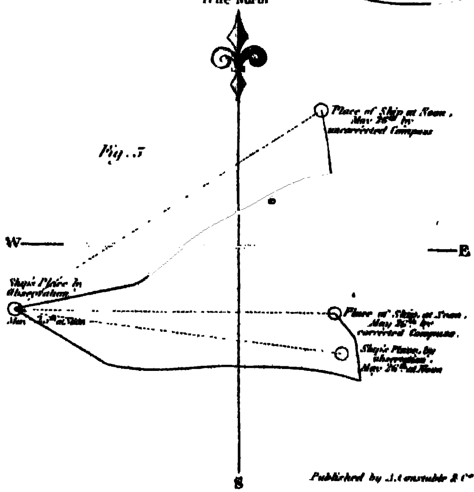


Fig. 5



Published by A. Constable & Co Edinr 1824.

these points of no action in the same plane? And, if so, is that plane parallel or oblique to the horizon? A series of experiments directed to this inquiry demonstrated, that the points were all in the same plane; and that this plane formed with the horizon an angle equal to the complement of the dip, descending from the north towards south. That this ought to be the case when the needle had its natural dipping position, might easily have been foreseen; because, then, the iron would be symmetrically situated with respect to the two poles of the needle; but that it should still be the same with the horizontal needle, was a fact as novel to the author, as it was important in all his subsequent pursuits. Having traced this circle on his iron-ball, and assuming the direction of the dipping needle as a principal axis to the same, its extremities forming the poles, he was immediately in possession of an ideal magnetic sphere, by which to indicate the relative position of the iron and compass in all his future inquiries; and to this happy idea he is doubtless indebted for the remarkable success with which his experiments have been attended.

The nature and properties of this ideal sphere, and the facilities it affords in all magnetical computations, will be more readily comprehended by referring to Fig. 1. Pl. II., in which O is supposed to represent an iron-ball, and AAA a supposititious sphere circumscribing it, and within which its influence is active; SCNQ′ being the magnetic meridian. The line NS in the plane SENW, denotes the natural direction of the dipping-needle in these latitudes where its inclination to the horizon is about 70°. Now, conceiving QEQ′W to represent a circle or plane passing through the centre of the ball, and perpendicular to the axis NS, it will be the *plane of no attraction*, or the *magnetic equator*; which, as we have seen, has this remarkable property, that if lines be drawn in it, (as for example, the lines OC, OC′, OC″, &c.), and a compass be placed any where in those lines, or, in short, in any point of the plane QEQ′W, it will be uninfluenced by the iron-ball, and preserve its natural magnetic direction. But, as soon as the compass is removed out of this plane, the needle is found to deviate from its original bearing; its south end being drawn towards the ball, when the needle is below the plane, and its north end when it is above; and in every case the

nature, and which, although simple in operation, was so intimately connected with one of the most intricate branches of natural philosophy. But, before entering farther into the results of these experiments, it will be proper to state a few particulars relative to the method of fixing the plate in its proper situation, and the construction of the plate itself.

In order to ascertain the situation for the plate, a place must first be selected, by the captain, for the azimuth or regulating compass to be fixed in for observation during the period of the ship being in commission. It will then be necessary to ascertain the local attraction of the vessel, which may be done in the following manner. The ship being moored, or lying with a short scope of cable, must have anchors so arranged, as to admit of her head being directed to each point of the compass successively, and there steadied, whilst the bearing of a remote object is taken (the more distant the better), to avoid the parallax, which would otherwise affect the observations. It will then be found, that the bearings thus observed differ from each other, according to the attractive power of the vessel, from 6 or 8 to 26 or 28 degrees; a difference which is caused by the iron of the ship attracting the needle out of its proper direction to the eastward, with the ship's head east, and to the westward with the head to the west. On examining these several bearings, there will be found two, at opposite points of the compass, that will nearly agree with each other, the mean of which must be accounted the true magnetic bearing of the object; and these points will also indicate the line of no attraction in the vessel, and which will generally be found nearly fore and aft: in this line the plate is ultimately to be fixed. By comparing the correct magnetic bearing, as before found, with the observed bearing at the several points, the amount of the local attraction, at each point, will be ascertained.

It now remains to determine the position of the plate, which hitherto has been a matter of experiment, by taking a pedestal or compass-stand on shore, and by trying different situations for it, and turning it round, till the same deviations were produced, by the plate at each point, as had been already observed in the vessel; but, in future, this will be more readily done, by means of a small printed table, which Mr Barlow intends to supply

with every plate for that purpose. This table comprises a series of attractions obtained by the plate, comprehending all possible limits for every class of vessels, and amongst them, of course, will be found that of any vessel in question; corresponding to which, are given two numbers, the one being the distance of the centre of the plate below the pivot of the needle, and the other its distance from the centre line of the pedestal; and at this depth and distance in the line of no attraction, already mentioned, the plate is ultimately to be fixed.

This being determined, the plate may now be placed either fore or aft of the compass; in the former case, doubling the effect of the vessel, and in the other neutralising it. When the former method is adopted, the plate is not a fixture, but is merely applied, whenever it is thought necessary to correct the course, or to take the variation of the compass by azimuth, or amplitude observations; in the other, it remains fixed in its place, during the voyage, and the needle thereby left free to obey only the magnetic power of the earth. The former is better suited for southern voyages, in which the local attraction is not great, and the latter, in northern voyages, where this disturbance is very considerable; it is, in fact, essentially necessary in the latter case, to preserve the due action of the needle, which, as we shall see, is enabled, by this means, to continue its action with the plate attached, after it would cease altogether to act without it; so that the experiment not only preserves the due direction of the needle, but it also renders it active after it would otherwise cease to traverse on its pivot. With respect to the plate, it has hitherto been made double; viz. of two plates screwed together, in such a manner, as to combine any strong irregular power of one with a like weak point in the other; by which means a more uniform attraction is obtained. Mr Barlow, however, seems to think that this precaution would not be necessary, if iron, weighing about 6 lb. per square foot, were employed; but with thinner plate-iron, viz. of about 3 lb. per foot, it is requisite, not only for the purpose above stated, but also to prevent any accidental bending, by a fall, or otherwise. The plates may vary from 12 to 16 inches in diameter, according to the power of the vessel. They have a hole in their centre, through which is passed a brass socket, with a broad head, and with an

exterior screw and nut, whereby the two plates, and an interposed plate of wood, of the same size, are compressed strongly together; the board being intended to increase somewhat the thickness, without adding much to the weight. It appears, also, that the two plates thus separated, are more powerful than when in immediate contact. The plates are afterwards more strongly attached to each other, by three brass pins and nuts. The several parts of the plates are represented in Figs. 2. and 3., and the brass-pin, socket, and pedestal in Fig. 4.; the latter of which shews the whole combined, as in action on shipboard.

Having thus fully described the nature of the operation, let us now examine the results that have been obtained in the vessels before named, and in the Griper, during the recent voyage of that vessel to Spitzbergen, in which we shall avail ourselves of the information contained in Mr Barlow's "Report, addressed to my Lords Commissioners of the Admiralty," as we have, in the preceding, been indebted to his "Essay on Magnetic Attractions," to which the above report now forms an appendix.

The first experiments, with the correcting plate, were made on board his Majesty's ship Leven, which sailed under the command of Captain Bartholomew, in April 1820, to the western coast of Africa, but returned the following year, under the command of Captain Baldey, in consequence of the death of the former officer. From Captain Baldey, and from the other officers of the vessel, Mr Barlow received a very extensive series of experiments, made with and without the plate, to determine the variations of the needle in those seas; which were accompanied by a letter from the commander, speaking in the highest terms of the efficacy of the method proposed. We cannot allow ourselves to transcribe these experiments at length, but the following observations on them by the author, will sufficiently explain the nature of the results.

After giving the tabulated series of the experiments alluded to, Mr Barlow observes, that "the best test we have of estimating the accuracy of the corrected variations, or the efficacy of the plate for this determination, is by comparing those variations with each other which were made on the same, or on subsequent days, while the latitude and longitude remained nearly the same;

first, as found in the usual way without the plate, and then with the plate, the ship's head being on opposite sides of the meridian; when although some difference (while the ship is changing her place) must be expected in the resulting variations, yet that change day by day will be but small, and we shall not fail to consider those results to be the most accurate, which agree nearest with each other." Several such examples are then selected out of the preceding table, as follows.

Experiments.	Difference in the variation without Plate.	Difference in the variation with Plate.	Experiments.	Difference in the variation without Plate.	Difference in the variation with Plate.
3 & 4	1° 5′	0° 23′	40 & 41	1° 53′	0° 40′
7 8	1 56	0 4	65 66	1 25	0 17
15 16	1 0	0 1	70 71	1 35	0 27
25 26	2 41	1 23	71 72	1 28	0 2
26 27	2 54	1 30	75 76	2 13	0 14
29 30	2 15	0 53	80 81	3 10	1 24
34 35	3 40	0 31			

These deductions are obviously perfectly satisfactory, as far as they go, but they are not a fair specimen of the value of the plate; for although, in the situation selected for the azimuth-compass, the local attraction did not exceed 3° or 4°, yet the compasses in the binnacle were at the same time at variance with each other to the amount of 5°, 6°, or 7°; and as it is by these that the vessel is steered, the actual correction in the course was to the same amount, as appears from the following example, furnished by Lieutenant Mudge.

" On the 22d of May, at noon, we were in latitude 41° 46′ N., and long. by chronometer 9° 53′ W. Taking this as our departure, we sailed by the starboard compass S. 46° W. 183 miles; this placed the ship on the 23d (allowing the variation 21° W.) in lat. 38° 58′ N., and long. 11° 26′ W. Whereas the observation at noon for latitude gave 38° 39′ N. and long. 10° 58′ W. So great a difference in 24 hours was attributed to a current, till I compared the steering or starboard compass with the one with your plate, when I found no less than 7° error to be subtracted from the course steered, making the true course S. 17° W., instead of S. 24° W., which had been taken

as correct. By allowing the 7° which we had found subtractive from the course, our latitude was by reckoning 38° 41′ N. and long. 11° 02′ W., which agree with observation as closely as we can ever expect to do under any circumstances."

Thus far, then, the experiments were found fully to answer their intended purpose, but an important question was still to be decided. Captain Flinders had observed, that, with an equal north and south dip, he found an equal quantity of local deviation; but in a contrary direction, the north end of the needle, in one instance, and the south in another, having been drawn forwards by the action of the vessel, it was, therefore, of the highest importance, to ascertain how far the power of the plate was competent to meet this strongly marked difference in the action of the ship. This was the point left for the decision of Captain Basil Hall, in his voyage in the Conway, round Cape Horn, to the western coast of America. We cannot do better, in describing these experiments, than by following our author's plan in transcribing Captain Hall's letter, written on the return of the vessel in 1822.

"In practice," says this officer, "the following is the method we pursued."

"A set, or several sets of azimuths, were taken without the plate, then another set or sets with the plate affixed; the ship's head, and all other circumstances remaining the same. The variation of the compass was then computed from each of these observations: now, that variation resulting from the first observations, was affected simply by the local attractions of the ship, and may be termed the *deviated deviation:* that resulting from the azimuths, when the plate was affixed, by an action twice as great, viz. first, by the ship, and, next, by the plate, may be termed the *double deviated variation.* The difference between these variations is the amount of the local attraction or the deviation, and this applied to the deviated variation gives the correct magnetic variation.

"It is easy to see how this correction is to be applied, by merely observing whether the north end of the needle has been drawn to the west or to the east, by the application of the plate, and considering that the ship's attraction must have had a similar effect on the needle.

"The following observations were made at sea by Mr Foster, under my superintendence and occasional assistance. The instrument used was an azimuth compass, made by Messrs W. and T. Gilbert of London, lent to me by the makers, at the suggestion of Professor Barlow. It is so constructed, that the observer reads off the angle at the same time that he observes the object: it is in other respects, also, admirably suited for practice, not only on such occasions as this, where much delicacy is required, but also in surveying and in piloting ships, by means of charts and bearings. The azimuth compasses at present supplied to his Majesty's ships (1822) are altogether unfit for any of these purposes, even the most common.

(Signed) "BASIL HALL."

Lieutenant Foster, the gentleman spoken of above, and who made the greater number of the observations under the superintendence of Captain Hall, gives in this place a detailed statement of all his experiments: they are continued from England, below Cape Horn, and hence, again, to the magnetic equator, on the western coast of America. They occupy several pages in Mr Barlow's report, but they are afterwards brought into a tabulated form, in order to bring them more collectively under the eye of the reader, and the better to compare them with each other, and with the observations of Captain Flinders; the author having added two columns for this purpose, one containing the dip of the needle at each place of observation, from Hansteen's chart, and the other shewing the end of the needle, that was drawn forwards, according to the indication of the plate, in order to ascertain how far the deductions agreed with those of the distinguished officer above alluded to. This Table, and the author's remarks upon it, must conclude our notice of these important observations.

Tabulated Results of Experiments made with Mr Barlow's Correcting-plate in H. M. S. Conway, by Captain Basil Hall and Mr Foster, in a Voyage to the Western Coast of America.

	Latitude.	Longitude.	Diff. by Hansteen's Chart.	Observed variation.	Corrected variation.	Local Attraction.	Direction of Ship's Head.	End of Needle drawn forward.
1	49° 30′ N	5° 15′ W	72° 0′ N	30° 6′ W	27° 46′ W	2° 20′ W	WSW	North
2	47 0 N	8 20 W	71 0 N	29 20 W	25 46 W	3 34 W	WSW	North
3	45 0 N	11 0 W	70 0 N	29 13 W	25 10 W	4 3 W	SW by W	North
4	43 30 N	12 0 W	69 0 N	28 11 W	25 40 W	2 35 W	SW by W	North
5	40 4 N	14 30 W	69 0 N	28 13 W	26 31 W	1 42 W	SW	North
6	36 11 N	14 53 W	65 0 N	23 56 W	23 58 W	0 2 E	South	...
7	35 11 N	14 0 W	65 0 N	21 20 W	21 28 W	0 8 E	SSE	North
8	30 7 N	15 47 W	63 0 N	23 7 W	21 6 W	2 1 W	SW	North
9	27 20 N	17 0 W	60 0 N	22 1 W	19 43 W	2 18 W	SW by W	North
10	26 20 N	18 0 W	60 0 N	21 52 W	19 52 W	2 0 W	SW by W	North
11	24 0 N	19 45 W	60 0 N	21 5 W	19 44 W	1 21 W	SW by W	North
12	21 40 N	21 40 W	55 0 N	19 43 W	18 44 W	0 59 W	SW by W	North
13	20 0 N	23 12 W	55 0 N	In three	observa	tions ...	......	North
14	18 30 N	24 45 W	53 0 N	17 10 W	16 12 W	0 58 W	SW by W	North
15	15 45 N	25 40 W	50 0 N	14 2 W	14 8 W	0 6 E	South	...
16	8 51 N	19 30 W	40 0 N	14 37 W	14 48 W	0 11 E	SE by S	North
17	0 30 S	24 0 W	25 0 N	12 31 W	12 31 W	0 0	SW	...
18	1 24 S	25 0 W	22 0 N	11 25 W	11 27 W	0 2 E	SW	South
19	9 50 S	31 45 W	9 0 N	6 13 W	6 29 W	0 16 E	S by W ½ W	South
20	14 0 S	33 15 W	0 0 S	4 28 W	4 50 W	0 22 E	S by W ½ W	South
21	15 52 S	34 0 W	3 0 S	3 47 W	4 17 W	0 30 E	S by W ½ W	South
22	18 40 S	36 40 W	3 0 S	0 46 W	1 6 W	0 20 E	SW ½ S	South
23	22 55 S	43 15 W	20 0 S	4 2 E	4 4 E	0 2 W	WSW	North
24	23 18 S	43 12 W	21 0 S	4 0 E	4 0 E	0 0	SSE	...
25	25 35 S	44 0 W	25 0 S	4 59 E	5 6 E	0 7 W	SSW ½ W	North
26	27 0 S	46 10 W	30 0 S	5 40 E	5 49 E	0 9 W	SSW ½ W	North
27	28 41 S	46 40 W	30 0 S	7 24 E	7 28 E	0 4 W	SSW	North
28	52 30 S	64 40 W	62 0 S	21 17 E	21 18 E	0 1 W	S by E	South
29	55 40 S	....	....	23 49 E	23 0 E	0 49 E	SW ½ W	South
30	...	....	....	26 28 E	24 32 E	1 56 E	SW	South
31	60 46 S	72 0 W	70 0 S	27 37 E	27 53 E	0 16 W	N by E	South
32	60 56 S	72 30 W	70 0 S	30 3 E	27 39 E	2 24 E	SW by S	South
33	60 36 S	77 45 W	70 0 S	30 31 E	27 47 E	2 44 E	W by N ½ N	South
34	57 38 S	84 10 W	70 0 S	28 18 E	26 1 E	2 17 E	WNW	South
35	43 20 S	79 30 W	65 0 S	18 50 E	18 26 E	0 24 E	N ½ W	South
36	39 7 S	78 0 W	57 0 S	17 16 E	17 12 E	0 4 E	N by E	North
37	36 30 S	75 40 W	50 0 S	15 57 E	16 11 E	0 14 W	NNE	South
38	12 3 S	77 5 W	....	9 37 E	9 50 E	0 13 W	S by E ½ E	South
39	12 27 S	78 0 W	....	9 26 E	9 14 E	0 12 E	SW	South
40	14 18 S	80 20 W	....	10 16 E	9 54 E	0 22 E	SW	South
41	18 57 S	85 0 W	....	10 10 E	9 50 E	0 20 E	SSW	South
42	23 30 S	87 52 W	....	10 26 E	10 26 E	0 0	S by W	...
43	18 28 S	70 15 W	....	10 25 E	9 47 E	0 38 E	SW	South

On examining the numbers contained in the above tabulated results, their general agreement with the deductions of Captain Flinders will be immediately obvious. That distinguished offi-

cer found, that, with equal dips, north and south, he had equal local attractions, but reversed in direction: and the whole of the foregoing table indicates the same change, the north end of the needle being drawn forward, while the dip is north, and the south when the dip is south, at least the exceptions are only in places near the magnetic equator, and the amount of the difference in these cases never exceeds a few minutes of a degree. The general decrease of effect from England to the equator, the increase again from the equator to Cape Horn, and the decrease thence as the southern latitudes diminish, are striking instances of the accuracy of the method of correction proposed. To which I may also add, as a still stronger case, the variations as found with and without the plate, in experiments 31, 32, 33, in which the greatest difference

Without the plate, is	. .	2° 53′
With the plate, only	. .	0° 14′

It is thus rendered obvious, that the plate, as fixed in Portsmouth Harbour, in Lat. 50° 47′ N., will correct the local attraction of a vessel in Lat. 60° 56′ S.; the dip in the former case being 70° north, and in the latter about the same south.

In short, it is rendered evident from the experiments made in the *Conway*, that the method of correction proposed is applicable through all navigable latitudes from 50° north to the highest approachable southern regions.

Only one point could now be considered as doubtful, respecting the efficacy of this method of correction. It had been ascertained by the observations of Captains Ross and Parry, that the effect produced by the iron of the ship increased with immense rapidity, and amounted to the most fearful quantity in approaching towards the pole: Would the plate increase in power with equal pace? To ascertain this point, Lieutenant Foster, who had already received the thanks of the Board of Longitude for the experiments on this and other scientific subjects in the *Conway*, was now appointed to the *Griper*, which was about to leave England for Spitzbergen, under the command of Captain D. C. Clavering, with orders to continue his experiments on local attraction under the superintendence of the above officer at every opportunity.

We are sorry we are not able to give the entire detail of these experiments, which are the more interesting as they were made

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	Latitude.	Longitude.	Diff. by Hansteen's Chart.	Observed variation.	Corrected variation.	Local Attraction.	Direction of Ship's Head.	End of Needle drawn forward.
	° ′	° ′	° ′	° ′	° ′	° ′		
1	49 30 N	5 15 W	72 0 N	30 6 W	27 46 W	2 20 W	WSW	North
2	47 0 N	8 20 W	71 0 N	29 20 W	25 46 W	3 34 W	WSW	North
3	45 0 N	11 0 W	70 0 N	29 13 W	25 10 W	4 3 W	SW by W	North
4	43 30 N	12 0 W	69 0 N	28 11 W	25 40 W	2 35 W	SW by W	North
5	40 4 N	14 30 W	69 0 N	28 13 W	26 31 W	1 42 W	SW	North
6	36 11 N	14 53 W	65 0 N	23 56 W	23 58 W	0 2 E	South	...
7	35 11 N	14 0 W	65 0 N	21 20 W	21 28 W	0 8 E	SSE	North
8	30 7 N	15 47 W	63 0 N	23 7 W	21 6 W	2 1 W	SW	North
9	27 20 N	17 0 W	60 0 N	22 1 W	19 43 W	2 18 W	SW by W	North
10	26 20 N	18 0 W	60 0 N	21 52 W	19 52 W	2 0 W	SW by W	North
11	24 0 N	19 45 W	60 0 N	21 5 W	19 44 W	1 21 W	SW by W	North
12	21 40 N	21 40 W	55 0 N	19 43 W	18 44 W	0 59 W	SW by W	North
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15	15 45 N	25 40 W	50 0 N	14 2 W	14 8 W	0 6 E	South	...
16	8 51 N	19 30 W	40 0 N	14 37 W	14 48 W	0 11 E	SE by S	North
17	0 30 S	24 0 W	25 0 N	12 31 W	12 31 W	0 0	SW	...
18	1 24 S	25 0 W	22 0 N	11 25 W	11 27 W	0 2 E	SW	South
19	9 50 S	31 45 W	9 0 N	6 13 W	6 29 W	0 16 E	S by W ½ W	South
20	14 0 S	33 15 W	0 0 S	4 28 W	4 50 W	0 22 E	S by W ½ W	South
21	15 52 S	34 0 W	3 0 S	3 47 W	4 17 W	0 30 E	S by W ½ W	South
22	18 40 S	36 40 W	3 0 S	0 46 W	1 6 W	0 20 E	SW ½ S	South
23	22 55 S	43 15 W	20 0 S	4 2 E	4 4 E	0 2 W	WSW	North
24	23 18 S	43 12 W	21 0 S	4 0 E	4 0 E	0 0	SSE	...
25	25 35 S	44 0 W	25 0 S	4 59 E	5 6 E	0 7 W	SSW ½ W	North
26	27 0 S	46 10 W	30 0 S	5 40 E	5 49 E	0 9 W	SSW ½ W	North
27	28 41 S	46 40 W	30 0 S	7 24 E	7 28 E	0 4 W	SSW	North
28	52 30 S	64 40 W	62 0 S	21 17 E	21 18 E	0 1 W	S by E	South
29	55 40 S	....	....	23 49 E	23 0 E	0 49 E	SW ½ W	South
30	...	....	....	26 28 E	24 32 E	1 56 E	SW	South
31	60 46 S	72 0 W	70 0 S	27 37 E	27 53 E	0 16 W	N by E	South
32	60 56 S	72 30 W	70 0 S	30 3 E	27 39 E	2 24 E	SW by S	South
33	60 36 S	77 45 W	70 0 S	30 31 E	27 47 E	2 44 E	W by N ½ N	South
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38	12 3 S	77 5 W	....	9 37 E	9 50 E	0 13 W	S by E ½ E	South
39	12 27 S	78 0 W	....	9 26 E	9 14 E	0 12 E	SW	South
40	14 18 S	80 20 W	....	10 16 E	9 54 E	0 22 E	SW	South
41	18 57 S	85 0 W	....	10 10 E	9 50 E	0 20 E	SSW	South
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On examining the numbers contained in the above tabulated results, their general agreement with the deductions of Captain Flinders will be immediately obvious. That distinguished offi-

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We are sorry we are not able to give the entire detail of these experiments, which are the more interesting as they were made

in a part of the world where the dip is very great, and where hitherto the compass has been considered as an useless instrument. Moreover, the attraction of this vessel, before leaving England, was very much greater than usual, so that on all accounts the plate was in this instance submitted to the most severe trial.

By a series of observations, made while the vessel was lying at the Nore, it was found that the bearing of a distant object differed 28°, with the ship's head at east and west; that is, the local attraction at each of these points was 14°, and proportionally great at all the other points,—an excess of attraction which Captain Clavering attributed to the effect of the spindle of the patent capstan, a suggestion which was verified by experiment on the return of the vessel, as we have already stated.

To counteract this strong power, it was necessary to bring the iron-plate, which was 14 inches in diameter, to a distance (from the middle of the pedestal) of $7\frac{3}{4}$ inches; and the centre of it $7\frac{1}{2}$ inches below the pivot of the needle, in which situation *abaft the compass*, it counteracted the local attraction of the ship, and left the needle free to obey the natural directive power of the earth.

This was proved by taking the variations of the needle with and without the plate, (as already explained in the *Conway* experiments,) from England to the North Cape; when the close agreement of the former, and the great discrepancy in the latter, were so marked, that from this time the vessel was navigated during the remainder of the voyage altogether by the corrected compass, and with the best possible success. She was moreover swung in three different ports during the voyage, viz. at Hammerfest, at Drontheim, and at Spitzbergen, and the local attraction ascertained at every point; first without and then with the plate: this was found to be at the east and west, or maximum points, without the plate, as follows, viz.

Hammerfest,	24° 10′
Spitzbergen,	34 42
Drontheim,	21 23
England,	14 00

Whereas, with the plate affixed, the deviations were reduced to quantities very little exceeding what might be attributed to errors of observation.

It will, however, be more satisfactory to state some of these results at length.

The following Table *shews the Variation as observed with and without the Plate, at different times during the Voyage.*

Latitude.	Longitude	Ship's head.	Variation without the Plate.	Variation with the Plate.	Time of observation.
65° 6′ N	6° 54′ E	North	26° 1′ W	24° 23′ W	May 18. 1823.
Ditto	Ditto	NE	11 28	25 2	Do. do.
66 57	7 20	North	24 52	25 30	May 20. 1823.
66 15	8 0	E ½ N	2 14	21 15	Do. do.
66 35	9 12	NE ½ E	11 58	22 43	May 21. 1823.
67 21	9 4	NE ½ E	18 4	22 12	May 23. 1823.
Ditto	Ditto	West	43 5	20 0	Do. do.
69 8	14 30	NE	13 35	13 35	May 28. 1823.
Ditto	Ditto	West	40 37	14 28	Do. do.

A single glance at this Table is sufficient, to perceive the very irregular character of the variations, as determined without the plate, and their close approximation to uniformity when the plate was affixed.

Extract from Journal of H. M. S. Griper.

H. M. S. Griper at Sea, 25th May 1823.								Lat. 69° 16½′ N. Long. by Chr. 7° 54′ E.
H.	K.	F.	Courses with Plate Compass.	Courses by Compass without Plate.	Winds by Plate Compass.	Lee-way.	Officers Initials.	Remarks, &c.
1	3	..	ESE	E by N		2 pts.		P. M.
2	3	..			NE			Fresh breezes and cloudy.
3	3	4					F. G.	4. Fresh breezes with a
4	3	4						head swell.
5	3	..						
6	3	..	E by S ½ S	E by N ½ N		ditto	T. D.	Variation 2 points west.
7	2	..						
8	3	..	E by S	ENE	NE by N	ditto	H. F.	8. Squally weather.
9	3	2	E by S ½ S	E by N ½ N		1 pt.		More moderate—set top-
10	3	2	E by S	ENE		ditto		gallant sails.
11	3	..	E ½ S	ENE		ditto	P. G.	Midnight, moderate, and
12	2	6						fine.
1	3	..	E by S	E by N ½ N		½ pt.		A. M. May 26. moderate
2	3	..	ESE	E by N	NE by N	ditto		and fine.
3	3	..						
4	3	..					T. D.	4. Fine clear weather—
5	2	6						set royals.
6	2	4	SE by E ½ E	E		ditto		6h 30m tacked.
7	1	..	N by E	N by E ½ E	E by N	None		
8	1	6					H. F.	8. Moderate and fine.
9	1	6						
10	1	6						Got spare sails up to dry.
11	2	..	N ½ W	N ½ E	ENE		P. G.	Variation 23° westerly.
12	2	..						Noon moderate and clear.
Lat. observed at Noon, 69° 12′ 10″ N.; Long. by Chr. 10° 14′ 15″ E.								

The preceding is a copy of an extract from the journal of the vessel, showing the courses, &c. as kept both by the corrected and uncorrected compass, and the amount of errors corrected by the plate.

The course, according to both reckonings, is shewn in Fig. 5. and the numerical results will stand as below:

Course and distance made good between the observations on the 25th and 26th of May 1823:

Course = S. 85° E., distance 50 miles.

By the Plate compass course = E. distance 51 miles.

By the Compass without the Plate, . . } Course = N. 58° E., distance 58 miles.

Latitude observed May 26. = 69° 12′ 10″ N.; Longitude by Chronometer 10° 14′ E.

Latitude by the Plate Compass = 69° 16′ 00′ N.; Longitude 10° 17′ E.

Latitude by the Compass without the Plate, } = 69° 47′ 00″ N.; Longitude 10° 11′ E.

Making a difference in the latitude of 35 miles.

The following Table shews the local attraction of the *Griper*, as ascertained by swinging her at Hammerfest, and the second Table shews the same when the plate was fixed.

Hammerfest, Latitude 70° 40′ *N. Longitude* 23° 45′ *E. Variation* 11° 26′ *W. Dip* 77° 15′ *N.*

Position of Ship's Head.	Compass or deviated Bearing of Object.	Correct Magnetic Bearing of Object.	Local Attraction.	Position of Ship's Head.	Compass or deviated Bearing of Object.	Correct Magnetic Bearing of Object.	Local Attraction.
	° ′	° ′	° ′		° ′	° ′	° ′
South	S 58 30 W	S 62 30 W	— 4 0	North	S 61 30 W	S 62 30 W	— 1 30
S by W	63 40	do.	+ 1 10	N by E	57 50	do.	— 4 40
SSW	67 0	do.	+ 4 30	NNE	53 40	do.	— 8 50
SW by S	70 50	do.	+ 8 20	NE by N	49 0	do.	—13 30
SW	....	do.	...	NE	43 0	do.	—19 30
SW by W	79 0	do.	+ 16 30	NE by E	42 30	do.	—20 0
WSW	81 40	do.	+ 19 10	ENE	41 0	do.	—21 30
W by S	83 0	do.	+ 20 30	E ½ N	38 40	do.	—23 50
West	86 40	do.	+ 24 10	E ½ S	38 20	do.	—24 10
W by N	87 0	do.	+ 24 30	E by S	37 0	do.	—25 30
WNW	85 30	do.	+ 23 0	ESE	39 40	do.	—22 50
NW by W	81 30	do.	+ 19 0	SE by E	41 40	do.	—20 50
NW	....	do.	...	SE	44 0	do.	—18 30
NW by N	77 0	do.	+ 14 30	SE by S	47 40	do.	—14 50
NNW	71 40	do.	+ 9 10	SSE	49 40	do.	—20 50
N by W	69 0	do.	+ 6 30	S by E	56 15	do.	— 6 15

Note.—The dip is supplied by Captain Sabine throughout.

Local Attraction with the Plate affixed.

Position of Ship's Head by Plate Compass.	Bearing of the Object by Plate Compass.	Correct Magnetic Bearing of Object.	Differences.	Position of Ship's Head by Plate Compass.	Bearing of the Object by Plate Compass.	Correct Magnetic Bearing of Object.	Differences.
South	S 64° 10′ W	S 62° 30′ W	+ 1° 40′	North	S 61° 30′ W	S 61° 30′ W	— 1° 0′
S by E	63 30	do.	+ 1 0	N by W	61 30	do.	— 1 0
SSE	65 30	do.	+ 3 0	NNW	61 40	do.	— 0 50
SE by S	65 30	do.	+ 3 20	NW by N	61 50	do.	— 0 40
SE	64 10	do.	+ 1 40	NW	62 50	do.	+ 0 20
SE by E	62 40	do.	+ 0 10	NW by W	63 30	do.	+ 1 0
ESE	61 30	do.	— 1 0	WNW	64 40	do.	+ 2 10
E by S	61 20	do.	— 1 10	W by N	65 0	do.	+ 2 30
East	60 0	do.	— 2 30	West	64 20	do.	+ 1 50
E by N	60 10	do.	— 2 20	W by S	63 40	do.	+ 1 10
ENE	62 30	do.	0 0	WSW	63 40	do.	+ 1 10
NE by E	63 30	do.	+ 1 0	SW by W	58 20	do.	— 4 10
NE	62 20	do.	— 0 10	SW	59 0	do.	— 3 30
NE by N	63 0	do.	+ 0 30	SW by S	58 20	do.	— 4 10
NNE	62 20	do.	- 0 10	SSW	60 0	do.	— 2 30
N by E	63 0	do.	+ 1 0	S by W	61 20	do.	— 1 10

These results will be found fully to bear out the representation made of them by Captain Clavering to the Admiralty, who, in his letter to John Barrow, Esq. says, "Having been directed by their Lordships to make trial of Mr Barlow's plate, under Mr Foster's direction, I forward that gentleman's report, which it will be unnecessary for me to comment upon, farther than to acknowledge the extreme practical utility of it, as found during the whole voyage; as when once fixed abaft the compass (thereby neutralising the effect of the iron on board), nothing farther was necessary than to allow for the variation of the place."

And, in his letter to the author, he says, after speaking of the great amount of the attraction in the Griper:

"Under such circumstances, it is obvious that the compass would have been altogether useless (as indeed it has always been admitted to be in these high latitudes), but for your valuable correcting plate, with which, as I have already stated in my report, we found the compass to which the apparatus was attached as serviceable in these latitudes as in any other."

What is here alluded to is an advantage attending the plate beyond what the author had foreseen, viz. that it not only causes the compass to work correctly, but it also enables it to

work longer, that is in higher latitudes, than it would otherwise do. This was observed as an experimental fact by Lieutenant Foster, while the vessel was on the coast of Greenland, where it was obvious, that when the plate was removed the needle was wholly inactive, but as soon as the plate was replaced, the needle again became serviceable. At this time the cause of this important effect of the plate was not known; but on the return of the ship, Lieutenant Foster having attentively reconsidered the subject, demonstrated it to be the necessary consequence of the common principle of the composition and resolution of forces; a discovery in the highest degree creditable to the acuteness and ingenuity of this rising young officer. On the coast of Greenland, the effect of the local attraction of the vessel amounted to 45° at east and west, so that the action of the iron at this time on the needle was equal to that of the earth; and the direction which the needle would at any time take up, was in the diagonal of the parallelograms described on lines representing these two equal forces; and the intensity of the same would, in like manner, be denoted by the diagonal of that parallelogram. Therefore, with the ship's head towards the north, the intensity would be greater than with the earth alone; with the ship's head at east or west, the intensity would be greater also in the ratio of $\sqrt{2}$ to 1, but when the head was towards the south, the iron of the vessel being opposed in power to that of the earth, the action of the needle is destroyed, and is wholly inactive and unserviceable for the purposes of navigation. Thus, let NE (Fig. 6. Pl. II.) represent the force which the earth exercises on the needle, and NS that belonging to the iron of the vessel, and NL the resultant of the two; it is obvious, after the angle SNE exceeds a certain quantity, that the resultant NL is less than either of the single forces, and which soon becomes too small to bring the needle home to its proper direction. But by opposing to the force NS another equal force due to the plate, as NP (Fig. 7.), then the resultant of the three forces is the single terrestrial force NE, and the needle will be as free to move as if no iron were in its vicinity.

This admirable property of the correcting plate leads Lieutenant Foster to form the most sanguine expectations that, in the ensuing voyage, he will have the satisfaction to see the com-

pass, during the passage of the Hecla and Fury through Lancaster Sound and Barrow's Straits, perfectly active, where hitherto it has been stowed below as completely unserviceable.

We must here conclude our account of these important experiments with the following extract from Mr Barlow's report :

"The importance of this principle of correction, even for the purposes of keeping the reckoning at sea, is sufficiently demonstrated in the two cases given by Lieutenants Mudge and Foster (pages 75. & 81.), where, in the former, the error by the common compass course was nineteen miles in latitude, and twenty-eight in longitude; while, by the corrected compass course, the error was reduced to two miles in latitude, and four in longitude; and in the instance furnished by Lieutenant Foster, the error in latitude alone was thirty-five miles in a run of only fifty, which almost wholly disappeared on the corrected course."

"I am aware," says the author, "that seamen depend very little upon the reckoning by compass, while they can make the requisite astronomical observations; but as it frequently happens that many days may pass without their obtaining such observations, it cannot but be of considerable importance to them, in such cases, to possess the means of approximating the nearest possible to their true place. It is not, however, at sea that this method is of greatest use; it is in narrow channels, in piloting ships by means of charts and bearings, and in marine surveying, that it finds its most valuable application : in these instances nothing can supply the place of the compass, and it cannot but be important in such cases that its directive power should be freed from all irregularity."

"Every reader, whether a nautical man or not, must be aware of the great amount of error, and fatal consequences, which might arise in a few hours to a vessel in the channel, in a dark and blowing night, having for its only guide a compass subject to an error of 14 degrees in opposite directions at east and west, the very courses on which she would be endeavouring to steer; and who can say how many of the mysterious wrecks which have taken place in the Channel are to be attributed to this source of error, of which the most recent, that of the Thames, Indiaman, is a serious example. This vessel, besides the usual materials,

guns, &c. had a cargo of more than 400 tons of iron and steel; and it may easily be imagined, that such a cargo would produce an effect on the compass at least equal to the Griper and Barracouta; and this alone would be quite sufficient to account for the otherwise unaccountable circumstance, that after having Beachy-head in sight at 6 o'clock in the evening, the vessel should have been wrecked upon the same spot at 1 or 2 o'clock in the morning, without the least apprehension of being at all near shore."

"These subjects are unquestionably deserving of the attention of the first maritime nation in the world; and I am willing to hope, that the labour and attention I have bestowed on this inquiry for the last five years, will be found advantageous to nautical science, and entitled to the favourable consideration of those public boards, who are its natural patrons and protectors."

We are gratified in having to state, that the Board of Longitude has expressed its opinion of the importance of these discoveries, by conferring on Mr Barlow the largest premium (£500) allowed by the late Longitude Act; at the same time stating, that this sum is not to be considered as any remuneration for the time and expences bestowed upon the inquiry, which is recommended to be considered by the Navy and Admiralty Boards, as distinct from the above reward. We doubt not that the other public Boards, independent of Government, but interested in the progress of navigation, will express their opinion of Mr Barlow's labours by similar marks of approbation.

After this sheet had actually been prepared for going to press, we learnt that Professor Barlow and Lieutenant Foster had instituted a very careful set of experiments, on board both the Hecla and Fury, first without, and then with the Plate. We understand that the results give the strongest confirmation of all that has been said above, and that the Plate affords a perfect correction; but we have not succeeded in obtaining the details, which, however, we hope to lay before our readers in the next Number.

We shall wind up with one observation, addressed to the commercial public,—namely, that, if any vessel be in future allowed to go to sea, and especially to high latitudes, without the precautions so clearly pointed out by Mr Barlow, the loss both of property and of lives, in the event of shipwreck, may in most cases, as we conceive, be fairly attributable to the owners.

In answer to a letter addressed to Professor Barlow, on the subject of his Plate, respecting which a great degree of interest has been expressed at Leith, Glasgow, and other ports in Scotland, he has informed us, that at present his Plate, with complete Instructions for its use, will soon be ready for sale, at Messrs W. and T. Gilbert's, in London; and that he would recommend to them to correspond with their agents at the different sea-ports, in order to make some arrangements for the supply of all ships. In the mean time, he authorises us to announce, that he is preparing for publication, in a popular form, a minute description of the method of trying the experiments, and of affixing the Plate. We need scarcely promise our readers to give our pages gladly to the circulation of instructions, contributing so directly to the improvement and security of navigation.

Art. VI.—*Analysis of Pinite from St Pardoux in Auvergne.* By C. G. Gmelin, M. D. of Tübingen. Communicated by the Author.

The near relation between Pinite and Mica has been observed by a great many mineralogists. Kirwan marked this mineral by the name of *Micarelle*, and Werner placed it close to mica. Bernharde, on the other hand, thought, that the genus Pinite should be joined to that of Tourmaline; but against this union most of its properties seem to militate.

Two different analyses of pinite have already been made known; the one by Klaproth, of pinite from the Pinistollen, the other by Drapier, of pinite from Auvergne. They found it to consist of the following ingredients:

	Klaproth.	Drappier.
Silica, - -	29·50	46.0
Alumina, - -	63·75	42·0
Oxide of Iron, -	6 75	2·5
	100 00	90·5

Klaproth himself seems not to have put much confidence in his analysis, which was made at an early period, as he has not admitted it into his contributions. The mineralogical affinity which was presumed to exist between pinite and mica, having thus received no confirmation from chemistry, I imagined that a repetition of its analysis would not be superfluous. For this investigation, I made choice of the pinite of St Pardoux, it being the only kind I could procure in sufficient quantity.

Its specific gravity was found $= 2{\cdot}7575 + 6\frac{1}{2}$ R. It is asserted in almost all mineralogical works, that the pinite cannot be melted before the blowpipe: this assertion is founded upon the examination of Klaproth, made upon the pinite of Pinistollen. I have had no opportunity of examining that variety under the blowpipe; but the pinite of St Pardoux melts on the edges into a glass full of blisters, when thin splinters of it are presented to the flame, although it does not melt into a globule. I have observed in general the same reactions as those which have been described by Professor Berzelius, in his Treatise on the Blowpipe; and I have only one circumstance to add, which, in so far as regards the geological relations of pinite, seems to deserve some attention. The pinite, when heated in a glass phial, gives out water of a disagreeable empyreumatic smell, which instantly blues reddened litmus, and therefore contains ammonia. It cannot be determined whether this ammonia is ready formed in the mineral, or is rather a product of the decomposition of some animal matter contained in it: the latter, however, would seem to be the more probable conjecture. It may be observed, that pinite never occurs in fresh rocks, but always, as for instance, in Auvergne, in a decomposed granite, upon which the volcanic mountains of that country rest; and upon this occasion I beg

leave to observe, that I have discovered a considerable quantity of ammonia in the natrolite of Hohentwiel, and in the porphyry-slate itself, in which the natrolite is found in veins. (*Gilbert's Annalen*, 1820, No. iv. p. 367).

Analysis.—*a.* 1·49 grains of pinite in coarse pieces, were reduced by ignition to 1·459 grains; 100 parts, therefore, would experience a loss of 1·410.

b. 2.5 grains of pinite, after having been reduced to an impalpable powder, were mixed with 12.5 grains of carbonate of barytes, and ignited; the mass cohered loosely, and assumed a green colour. When dissolved in muriatic acid, chlorine was evolved. Silica, separated in the ordinary way, weighed, after ignition, 1·38 grains = 55·200 per cent.

c. The liquor was now precipitated by carbonate of ammonia; the precipitate which fell down was separated by the filter, and well washed. The solution was then evaporated to dryness, and the dry mass fused. Alcohol and a little muriatic acid were poured upon the fused mass, and the alcohol set on fire; but there appeared nothing of a green or purple colour, a circumstance by which the absence of boracic acid and lithium is proved.

d. The muriate of alkali converted into a neutral sulphate, weighed 0·387 grains; when dissolved in water, 0·0191 grains. Silica = 0·764 per cent. were left undissolved. The sulphate was now converted into a carbonate by means of acetate of lead, which was dissolved in water, leaving a small residue of oxide of manganese. The solution was saturated by muriatic acid, and evaporated, in order to drive off the excess of acid. The muriatic salt was dissolved in a little water, and mixed with a concentrated solution of muriate of platina. The precipitate was washed with a small quantity of water. The solution which passed through the filter was mixed with some sulphuric acid, evaporated, and exposed to an intense heat. The sulphate was separated from the metallic platina by means of water, and put aside for crystallization. There were formed crystals of sulphate of soda, which effloresced perfectly on exposure to the atmosphere, and weighed in this state, 0·022 grains. This quantity being deduced from the whole quantity of the salt (0·387 grains), there remain for the sulphate of potash 0·365 grains. 2.5 grains

of pinite contain, therefore, 0·19735 grains of potash = 7·894 per cent., and 0·00964 grains of soda = 0·386 per cent.

e. The precipitate formed by carbonate of ammonia (*c*) was dissolved in muriatic acid; and the barytes precipitated by sulphuric acid. The filtered liquor was then evaporated, again dissolved in water, mixed with caustic ammonia, and quickly filtered. The filtered liquor, when evaporated, deposited alumina, which, after ignition, weighed 0·038 grains = 1.52 per cent. Oxalate of ammonia produced in this liquor a very slight precipitate, which could not be weighed. When boiled with carbonate of potash, no precipitate fell down.

f. The precipitate thrown down by caustic ammonia (*e*) was dissolved in muriatic acid, and boiled with an excess of caustic potash. The alkaline solution was supersaturated by muriatic acid, and the alumina then precipitated by carbonate of ammonia. It weighed, after ignition, 0·899 grains, = 23·960 per cent.

g. The residue (in *f*) left undissolved by potash, was dissolved in muriatic acid, boiled with nitric acid, and the iron thrown down by succinate of ammonia. The oxide of iron weighed 0·1378 grains = 5·212 per cent.

h. The iron having been separated, the liquor was boiled with subcarbonate of potash, by which magnesia was precipitated, containing a quantity of oxide of manganese. It weighed, after ignition, 0.094 grains = 3·760 per cent.

The pinite of St Pardoux is therefore composed of

Silica,	55.200 (*b*) 0·764 (*d*)	55·964
Alumina, (With Traces of Lime),	23·960 1·520	25·480
Potash,		7·894 (*d*)
Soda,		0·386 (*d*)
Oxide of Iron,		5·512 (*g*)
Magnesia, with Oxide of Manganese,		3·760 (*h*)
Water, with an Animal Matter,		1·410 (*a*)
		100·406

Search for Fluoric Acid.—As all micas which occur in granite, contain a quantity of fluoric acid, I thought it necessary to inquire whether pinite contains this acid or not. This inquiry was made upon a quantity weighing 1·945 grains, according to

the method used by Berzelius in the analysis of topaz. A very slight precipitate was thrown down by means of muriate of lime and caustic ammonia, which was composed of alumina and silica. It was treated with sulphuric acid in a platina crucible, upon which a plate had been put, which was covered with a stratum of wax, and bared in some places; but there appeared no traces of etching, when, after standing some hours upon a warm furnace, the glass had been taken off and washed.

It would be useless to give a mineralogical formula for the composition of this mineral. For this purpose its purity would require to be better ascertained than was in reality possible. I have only to observe that the quantity of oxygen in silica is nearly double that in the other bases together.

The preceding analysis, I think, proves such an affinity between the pinite and mica, in regard to chemical composition, that they can no longer be considered as generically separate. The circumstance that pinite contains no fluoric acid, cannot be considered as an essential difference, when it is observed that even those varieties of mica which occur in primitive limestones, contain very little of that acid, or are entirely destitute of it, according to the experiments of H. Rose: fluoric acid ought not therefore to be considered as an essential constituent of mica. It is possible that the pinite, during the unknown alterations which it has evidently undergone, may have lost its original fluoric acid, or that this substance may have combined with an animal matter.

Art. VII.—*On the Geognostical Phænomena at the Temple of Serapis.*

[The singular phænomena exhibited at the Temple of Serapis have for a long period engaged the attention of geologists. In the Illustrations of the Huttonian Theory (§ 397, 398) the following observations occur, from which it appears that Breislac considers them as connected with changes in the level of the ocean, while Professor Playfair views them as illustrating risings and sinkings of the land.

" On the southern coast of Italy similar facts have been observed. Breislac, in his *Topographia Fisica della Campania di Roma*, from certain appearances in the gulfs of Bajia and Naples, concludes, that, at the beginning of the Christian era, the level of the sea was lower on that part of the coast than it is now. The facts which he mentions are the following: 1*mo*, The remains of an ancient road are now to be seen in the Gulf of Bajia, at a considerable distance from the land; 2*do*, Some ancient buildings belonging to Porto Julio are at present covered by the sea; 3*tio*, Ten columns of granite at the foot of Monte Nuovo, which appear to have belonged to the Temple of the Nymphs, are also nearly covered by the sea; 4*to*, The pavement of the Temple of Serapis, now somewhat lower than the high-water-mark, though it cannot be supposed that this edifice when built, was exposed to the inconvenience of having its floor frequently under water; 5*to*, The ruins of a palace, built by Tiberius, in the Island of Caprea, are now entirely covered by the sea.

" Thus it appears that the level of the sea is sinking in the more northern latitudes, and rising in the Mediterranean, and it is evident that this cannot happen by the motion of the sea itself. The parts of the ocean all communicating with one another, cannot rise in one place and fall in another; but, in order to maintain a level surface, must rise equally, or fall equally, over the whole of its extent. If, therefore, we place any confidence in the preceding observations, and they are certainly liable to no objection, either from their own nature or the character of the observers, we must consider it as demonstrated, that the relative change of level has proceeded from the elevation or depression of the land itself. This agrees well with the preceding theory, which holds, that our Continents are subject to be acted upon by the expansive forces of the mineral regions; that, by these forces, they have been actually raised up, and are sustained by them in their present situation.

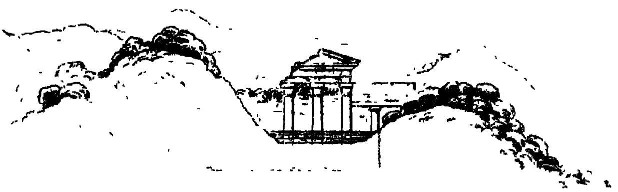
" According to some other facts stated by the same ingenious author, it appears that, on the coast of Italy, the progress of the sea in ascending, or of the land in descending, has not been uniform during the period above mentioned, but that different oscillations have taken place; so that, from about the beginning of the Christian era, till some time in the middle ages, the sea rose to be sixteen feet higher than at present, from which height it has descended till it became lower than it is now, and from

PLATE III.

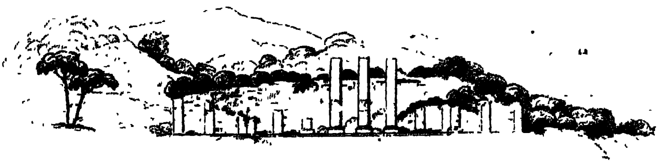
TEMPLE OF JUPITER SERAPIS AT PUZZOLO

Edin^r Phil. Jour. Vol. XI. p

Temple before its destruction



Temple partially covered with Volcanic substances



Temple after partial removal of the Volcanic substances

W.H. Lizars sc.

Published by A. Constable & C^o. Edin^r. 1824.

that state of depression it is now rising again. Breislac infers this from two facts, which he combines very ingeniously with the preceding; viz. the remains of some ancient buildings, at the foot of Monte Nuovo, five or six feet above the present level of the sea, in which are found the shells of some of those little marine animals that eat into stone; and again, the marble columns of the Temple of Serapis, which are also perforated by pholades, to the height of sixteen feet above the ground. All these changes Breislac ascribes to the motion of the sea itself; a supposition which, as we have seen, cannot possibly be admitted, since nothing can permanently affect the level of the sea in one place, which does not affect it in all places whatsoever."

The interesting account of this Temple in the following communication, translated from Göthe's *Morphologie*, maintains, that the appearances are not connected either with a change on the level of the sea, or with sinkings or risings in the land, but with some local alterations induced by volcanic agency.—Edit.]

After my return from Sicily to Naples, I found much still remained for me to overtake, which, in the bustle of other affairs, had been neglected; and, among other things, the Temple of Jupiter Serapis at Puzzuolo, the remaining pillars of which have long been remarkable, as presenting an inexplicable phænomenon to the naturalist.

We went thither on the 19th of May 1787. I examined every thing minutely, and very soon determined with myself how the appearances were to be explained. What I then noted in my journal, with all that has since come to my knowledge, I shall here set down in due order, in reference to a very accurate engraving.—See Plate III.

The situation of the Temple, or, more properly speaking, of its remains, is to the north of Puzzuolo, about 200 toises distant from the city, immediately upon the shore, and about 15 feet above the level of the sea.

The building still occupies a space of 25 toises square, but from that are to be deducted the cells of the priests all round, leaving an inner court, with its colonnade, of 19 toises in extent. In the centre there is a circular elevation, 10½ toises in diameter, ascended by four steep steps, which supported upon pillars a round open temple without cells.

The number of these amounted to sixteen. Thirty-six surrounded the court, and since a statue was allotted to each, there must have been fifty-two of these included in this moderate space. Let the whole be conceived of the Corinthian order, as is proved by the proportions of the columns, and the cornices lying scattered about, and it will be conceived that the effect must have been imposing and magnificent. This must have been still heightened by the solid masses, as well as by the inner coating, being all of the finest marble; and the cells of the priests, with the curious chambers for purification, were all found to have been inlaid, paved, and fitted in the same manner.

All these indications, and particularly the plan of the building, attentively considered, point rather to the third than the second century; but we are no longer able to pronounce any opinion grounded on the architectural ornaments, which would here prove most decisive.

Yet more uncertain is the period when the Temple was buried under volcanic ashes, and other ejected matter. We shall, however, give an account, in reference to the engraving, of what is still to be seen, as well as what may legitimately be inferred to have existed.

The first figure is a transverse section of the Temple, while entire. The four high columns stood in the middle of the court in front of the sanctuary; farther behind is seen the court, with its colonnade; and, last of all, the chambers of the priests.

It is not wonderful that the Temple came to be choked up at some uncertain period of the middle ages. Let any one take the plan of the Campi Phlegræi, and observe crater on crater, and the continual succession of elevation and depression, and he will easily be convinced that the ground here has never been at rest. The Temple is only an hour and a half's distance from Monte Nuovo, which, in September 1538, ascended to the height of 1000 feet, and only half an hour from the Solfatara, which is still in activity.

The middle figure represents the cells of the priests, covered by the thick showers of ashes, which thus form swells or hillocks, while the open court is only filled to a certain depth. There would thus be formed a hollow in the centre, only twelve feet elevated above the former level, out of which those of the

centre columns that remained standing, and perhaps also the upper part of the colonnade, would be seen projecting.

The stream which was conducted into the Temple for purification, to which the gutters and pipes since dug out, and the singularly cut marble slabs, bear sufficient testimony, and which still flows past at no great distance, being obstructed, would form a small lake, which may be supposed to have risen to the height of five feet, and to have washed the columns of the portico to this extent.

In this water was generated a kind of muscle, of the genus Pholas, which perforated the Grecian Cipolin marble all round, and quite upon a level with the water.

It is unknown how long this treasure remained concealed. It is probable the walls became covered with bushes, and, indeed, the whole country is so full of ruins, that the few projecting columns would scarcely attract notice.

At last, however, some modern architects made here an acceptable discovery. The water was led off, and an excavation undertaken; not to restore this ancient monument, but rather to use it as a quarry, and the marble was actually employed in building Caserta, which was begun in 1752.

This is the reason why there are so few regular remains to be seen; and hence the three columns standing on the cleared pavement particularly arrest the attention. These, to the height of 12 feet from the ground, are seen quite free and entire; and the next 5 feet in ascent is what has been perforated by the muscles before alluded to. On more minute inspection, the depth of the holes thus excavated has been found to be 4 inches; and the remaining shells have been extracted quite perfect.

Since the excavations for the purpose of obtaining building materials were discontinued, these ruins appear to have undergone no farther change; for, in a work, intituled "Antichità di Puzzuolo," in folio, with plates, published without a date, but dedicated to Ferdinand IV., on his marriage with Caroline of Austria, 1768, the state of the ruins is represented in the fifteenth plate, very nearly as we found them, and as exhibited in a drawing by Verschaffelt, 1790, which is preserved in the Library of the Archduke at Weimar.

The important work, "Voyage Pittoresque ou Description des Royaumes de Naples et de Sicile," contains also an account of this Temple, in the Second Part. The text is valuable, and imparts much varied information, though leading to no result. There are figures opposite page 167, drawn from hasty sketches, and arbitrarily finished to please the eye, but yet not far from the truth.

Less praise can be bestowed on the restoration attempted at page 172. of the same work. It is only, as is indeed confessed by the editors, a phantastical theatrical decoration, much too extensive and colossal, since the whole of this sacred edifice, as indicated by the dimensions, was executed in very moderate proportions, although adorned to superfluity.

Any one may be convinced of this from the ground-plan, given in the sixteenth plate of the Antichità di Puzzuolo, and which is copied at the 170th page of the Voyage Pittoresque.

From all this it appears, that there is here a good field opened for the exertions for an intelligent and experienced architect. A more accurate measurement than we are able to give, by revising the ground-plan, according to the data furnished in the works above cited,—a minute survey of the scattered ruins,—a critical judgment of the style which might determine the period of its erection,—and a restoration of the whole, and of its parts, according to the rules of art, and in the taste of the period in which the edifice was reared, are problems which yet remain to be solved.

The labours of the antiquary would thus be facilitated, who, on his part, would have to ascertain the kind of worship here practised. It must have been bloody, for there are still iron-rings in the pavement, to which the animals were tied; and for carrying off the blood, there are gutters all round; nay, there is even found in the centre of the middle elevation, a similar opening, through which the blood might be conveyed away. All this appears to us to point to a later period, of a mysterious and dark idolatry.

I now return to the main point, viz. the holes of the shell-fish or Pholas, which are indisputably to be ascribed to animals of this genus. How they reached that height, and have only perforated a certain portion round the columns, may be gathered

from the explanation of the plate given above. It is local, and sets the matter in a clear light, by the simplest means, and will certainly meet with approbation from all candid inquirers into nature.

The difficulty here seems, as is often the case, to proceed from a false supposition. The columns, it is said, are perforated by mussels of the genus Pholas, which exist only in the sea: the sea, therefore, it is inferred, must have risen to this height, and encircled the columns.

Such a conclusion requires only to be reversed; and we must infer, that, because the effects of these animals are here found more than 30 feet above the level of the sea, and a temporary lake can be shown to have existed, these mussels, of whatever species, must have been capable of subsisting in fresh water, or at least in water impregnated with salt by volcanic ashes. And here, I pronounce in general, without hesitation, that an explanation, supported by new experience, is worthy of consideration.

On the contrary, let the Mediterranean be supposed, in the dark period of priestcraft and knight-errantry, to have been elevated 30 feet above its former level, what changes must not the whole of its shores have undergone! How many bays enlarged,—how many tracts of country desolated,—how many havens destroyed! And besides, the water must have remained for a considerable period at this elevation; and yet, in no chronicle, in the history of no Prince, nor town, nor church, nor cloister, is there the slightest mention of such an occurrence, although accounts and traditions are nowhere entirely wanting in any of the centuries subsequent to the fall of the Roman Empire.

Here we may be interrupted by the question, What are you disputing about? And with whom? Has any one ever maintained that such a commotion of the sea took place since the Christian Æra? No, it belongs to a more early period, perhaps even to the Poetic Ages.

So be it. We give in willingly, since we have no wish to engage in a war of words; it is enough for us, that a Temple, built in the third century, can never thus have been inundated by the sea.

And therefore, I shall only, in reference to the engraving, (Pl. III.) resume what has been advanced, and subjoin a few remarks. Both in the upper and undermost figure, *a* is the level of the sea, and *b* the elevation of the Temple above it.

The centre figure represents the views we entertain. The line *c* indicates the filling up of the court of the Temple, which became afterwards the bottom of the lake; and *d* the height at which the water stood in it. Between these two points the mussels took up their abode. The mound formed above and around the Temple, at the period of its being overwhelmed, is denoted by *e*; and the columns and walls in the transverse section of the terrain are traced by dotted lines.

In the under figure, where the cleared spaces are seen, the perforated part marked by dots, corresponds to the former lake *c*, *d*, and sets our explanation in a clear point of view. We have only to remark, that the exterior mason-work of the Temple will not be found so open as it is represented, for the sake of conformity, but is covered with rubbish, since the excavation was continued only so long as it answered the purpose for which it was begun.

Were it necessary for me to add any thing more, it would be to give the reasons why I have not come forward with this explanation long ago. In this, as in other instances, I had convinced myself, and did not feel called upon, in this cavilling world, to undertake to convince others. When I published my Travels in Italy, I omitted this part of my Journal, because such a detail appeared to me unsuited to that work; and there, also, the matter was merely noticed, and illustrated by a few strokes of the pen.

But lately two circumstances have combined to induce me to make this disclosure, and to furnish me with the means of doing so. An equally friendly as expert architect, drew, after my slight hints, these parallel figures, which set the matter in a clear light, without any laboured explanation, and which, as they have been very neatly engraved by Schwerdtgeburth, will give satisfaction to all the friends of science.

I was also excited by the hope, that Von Hof might take notice of the present case, in his invaluable work, by which he saves the intelligent inquirer so many unnecessary questions, in-

vestigations, inductions, and answers. He diffidently separates the problematical parts, and wishes a less desperate explanation than the elevation of the Mediterranean for such an insignificant purpose. Let the present Essay, then, be first dedicated to this worthy man, with the reservation of being allowed, on other and more important occasions, without bias, to express the thanks which are due for his great and comprehensive undertaking.

Art. VIII.—*Experiments on the Application of Professor Dœbereiner's recent Discovery, to Eudiometry* *. By Edward Turner, M. D., Fellow of the Royal College of Physicians, and Lecturer on Chemistry, Edinburgh. (Communicated by the Author.)

Having observed with equal pleasure and surprise the singular property of spongy platinum, recently discovered by Professor Dœbereiner, it occurred to me to investigate whether this discovery was to afford simply an amusing experiment, or to confer a benefit on science, by the addition of a new agent in analytical chemistry. My preliminary trials were flattering, and I therefore resolved to enter on the investigation with all possible regard to accuracy.

Pure platinum was dissolved in the usual way, and precipitated by a solution of sal-ammoniac; the yellow precipitate, on being ignited, gave metallic platinum in the form of a delicate porous mass. When a jet of hydrogen-gas was thrown upon this mass, it became instantly red hot, and in a few seconds the heat was so intense as to inflame the hydrogen. The effect was greatly augmented, by throwing a jet of oxygen upon the platinum at the same time. A most intense white heat was then produced, accompanied by vivid combustion of the hydrogen, forming a splendid and striking experiment.

If the platinum be brought in contact with a mixture composed of two volumes of hydrogen to one of oxygen, instantaneous action ensues; the metal becomes red hot, and an explosion follows.

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* Read before the Royal Society of Edinburgh, on the 5th of April and 3d of May 1824.

Of course, it would be hazardous in the extreme to try this experiment on a considerable quantity of gas. The explosion does not occur immediately on the introduction of the metal into the mixed gases; the combination takes place at first silently and gradually, though rapidly; the metal is in consequence heated, then becomes dull red, at last passes to bright redness, and at the same moment the remainder of the gases unites with an explosion. This succession is commonly so rapid as to escape observation, but I shall afterwards relate an experiment which shows the order distinctly. It hence appears that the platinum differs somewhat in its mode of action from electricity; the latter agent causes the whole mass of gases to unite at once, and an explosion is the necessary consequence; whereas the platinum causes them to explode, not by its primary action, but through means of the heat disengaged by that action. Thus, although platinum, in its state of greatest activity, will generally cause an explosion, when brought in contact with $\frac{1}{4}$th cubic inch of a very pure explosive mixture; yet, if the quantity be still less, there is no explosion, since, before heat enough is developed for this effect, the whole gas is consumed.

When spongy platinum is put into a mixture of hydrogen and atmospheric air, a copious dew condenses on the surface of the mercury, and this effect was observable, even when the hydrogen bore a small proportion to the whole quantity of gas operated on. Several very interesting points of inquiry were suggested by this fact; but, before proceeding to their investigation, I found it necessary to avoid certain inconveniencies attached to the employment of spongy platinum, as prepared by the ordinary process. Its delicacy, for instance, is such, that it often falls to pieces from the pressure necessary to pass it under the mercury; or, at all events, its porous texture is so much injured from this cause, as to render its action irregular. Another serious objection to its employment in this state, is the facility with which the mercury acts upon it, particularly when heated. I observed this fact in a very marked manner on the following occasion. I had warmed the platinum gently with the view of increasing its energy; but it was no sooner plunged under the mercury than an amalgam formed, which sunk instantly to the bottom of the trough. On driving off the mercury by

heat, I obtained the platinum in a very altered condition; for it was firm, and had contracted to at least one-half of its former volume. For these reasons, I had recourse to the balls proposed by Professor Dœbereiner. I mixed together spongy platinum and pipe-clay, in different proportions, and, by the addition of a little water, made a set of balls, about the size of a pea. As the activity of spongy platinum is in some way connected with its porous texture, I endeavoured to bring the balls as nearly into a similar condition as possible. With this intention, a little powdered sal-ammoniac was mixed with the platinum and pipe-clay, and when the balls had become dry, they were cautiously ignited at the flame of a spirit-lamp. The sal-ammoniac escaping from all parts of the ball, gave it a degree of porosity which was very favourable to its action. In this way twelve different balls were formed, the exact composition of which I subjoin.

		Platina.	Pipe-clay.	
No.	1. consists of	5 grains,	1 grain.	
	2.	4	1	
	3.	3	1	
	4.	4	2	
	5.	4	4	
	6.	2	4	
	7.	2	6	
	8.	1	5	Silica
	9.	0·25	0·5	1.25
	10.	0·5	1 5	3
	11.	0·25	2.0	1
	12.	0·25	2.0	1·5

Siliceous earth was added to the four last, since the pipe-clay alone made the mass too tenacious. I found that any of the four first balls, the weakest of which contained ⅔ds its weight of platinum, might be very well substituted for the pure spongy metal. Freshly ignited, and allowed to cool, they became red when a jet of hydrogen was thrown upon them, and the gas itself then took fire. Put in contact with a mixture of two measures of hydrogen to one of oxygen, they became red, and an explosion followed, though not quite so rapidly as when spongy platinum was used. For example, when I put the ball No. 2. in 1·5 cubic inch of the explosive mixture, a dense dew formed on the mercury; rapid diminution succeeded, and in one or two seconds a dull light ap-

peared, which passed rapidly to vivid redness; but by this time all the gas was consumed. With ball No. 3., the same results were obtained. Had I operated in these cases on a larger quantity of gas, there must have been an explosion. To prove this, No. 4. was put in 4 cubic inches of the explosive mixture; absorption with dense watery vapour first appeared: in two or three seconds the ball became dull red, and passed rapidly, but during an appreciable space of time, to vivid redness, at which moment an explosion happened. The activity of these balls is greatly increased by heating them gently; I have known No. 4. in that case inflame an explosive mixture of less than half a cubic inch.

The balls No. 5, 6, 7, and 8., though not so powerful as the preceding, acted, nevertheless, with considerable energy. I put No. 8., for example, in four cubical inches of the explosive mixture, expecting only a silent action; to my surprise it became red, and caused an explosion. Nor would I venture to put Nos. 9, 10, and 11. in contact with much of the explosive mixture, for they act with an energy by no means to be expected from the small quantity of platinum present. No. 12. was put into a half cubical inch of the explosive mixture: a very sluggish action ensued, but in the space of five minutes the gases were completely condensed.

These experiments will convey a distinct idea of the extraordinary power of platinum, in causing the combination of oxygen and hydrogen gases, even when the quantity of metal present does not exceed one quarter of a grain, and that made up into a ball with fourteen times its weight of foreign matter. It is necessary to mention, that the gases I used in these experiments were of great purity, quite dry, and mixed in the exact proportion to form water. The oxygen was carefully prepared from dry chlorate of potash, and received over mercury. I prepared the hydrogen from zinc and dilute sulphuric acid, and collected it in a gas-holder over water. Successive portions of it were drawn off when required, and dried over mercury by fused potash. In the more delicate experiments I always ascertained its purity, and made due correction for the air it contained. If the gases are not mixed in the proper proportion to form water, a silent but rapid combination is produced. I have never had any

explosion when one volume of hydrogen was mixed with four of oxygen, nor when one of oxygen was added to four of hydrogen. Atmospheric air and hydrogen may be mixed in any proportion without danger of explosion.

The ball itself suffers no chemical change; at least I have used the same one in fifty different experiments, without observing the least diminution of its energy. Its activity does indeed diminish after repeated use, if not ignited during the experiments, but is completely restored by heating the ball to redness. The method I pursued in all the succeeding experiments, was to throw the flame of an oil or spirit lamp upon the ball, by means of a blowpipe, so as to keep it intensely hot for the space of one minute, after which it was allowed to sink to the temperature of the room before being used. Sometimes, however, it was necessary to plunge the ball through mercury while strongly heated, and this occasionally destroyed its action most completely; though its energy was restored by keeping it for a few minutes in the strongest heat of the blowpipe. I have sometimes observed the energy of an active ball so completely destroyed by passing it through mercury, that it did not produce the slightest effect in a highly explosive mixture.

Eleven volumes of atmospheric air were mixed with one volume of hydrogen. An electric spark was passed through one portion of the mixture, and the ball No. 3. brought in contact with another. Detonation occurred in the first case, and in the second, diminution of the volume of gas, with deposition of water. A strong charge of electricity was passed through a mixture, composed of 13 air to 1 hydrogen, without any detonation; whereas the balls, Nos. 3. and 5., afterwards caused immediate diminution. Repeated strong charges from a Leyden jar were sent through a mixture of 15 air to 1 hydrogen, without any detonation, while balls Nos. 3. and 5. acted instantly. A mixture, composed of 19 air to 1 hydrogen, was not at all influenced by electricity; but balls Nos. 3. and 7. caused formation of water, as did No. 8. when warm. The ball No. 3. was put into a mixture, composed of 1 volume of hydrogen to 4 of air: an abundant deposition of water followed; and in one minute after the entrance of the ball, the residual gas was transferred into a eudiometer and electrified, but without any deto-

nation. The very same occurred in a mixture of 5 air to 1 hydrogen. Ball No. 3. had a distinct action in mixtures of 30 and 40 air to 1 of hydrogen.

It is then apparent from these experiments, that the platinum balls caused the formation of water, when a strong electrical charge did not do so. It appears, too, that the balls acted so powerfully in a mixture of air and hydrogen, that electricity, afterwards employed, had no effect. The platinum is likewise shown to act with great velocity; for, after the short space of one minute, so large a quantity of hydrogen had disappeared, that the residual gas was not explosive. I have, within a few days, seen a short article by Professor Gmelin of Tübingen, published in a recent Number of the *Annales de Chimie et de Physique*, in which he states, that a mixture of air and hydrogen, after it had been acted on by platinum, still contained so much hydrogen as to detonate from the electric spark. He concludes, therefore, and justly, were his experiments correct, that platinum cannot be employed in eudiometry. He does not mention how he operated, and therefore I cannot perceive the source of his error.

I now proceeded to inquire, whether the exact quantity of hydrogen was indicated by the action of platinum.

In a mixture of 5 air to 1 hydrogen the diminution occasioned by an active ball indicated within a fraction the quantity of hydrogen present; and in a mixture of 9 air to 1 hydrogen the quantity of the latter was again correctly indicated.

These experiments were performed once and again with similar results.

In mixtures of 13 air to 1 hydrogen, 15 to 1, 17 to 1, and 19 to 1, the quantity of hydrogen was very accurately indicated, and the brilliancy of the mercury round the ball was in each case sullied by the formation of dew.

I mixed 5 volumes of hydrogen with 500 of air, or in the exact proportion of 1 : 100. The ball No. 2., gently warmed, was put into 180 volumes of this mixture, which, of course, contained 1.8 of hydrogen. In half an hour there was diminution of 3 volumes, which indicates the presence of 2 hydrogen.

The ball No. 2., gently warmed, was put into 186 volumes of the same mixture, containing of course 1.86 hydrogen. In three quarters of an hour there was a diminution of 2.8, which indicates the presence of exactly 1.86 hydrogen.

No. 2. gently warmed, was put into 132 volumes of the same mixture, containing 1.3 volumes of hydrogen. In three quarters of an hour there was a diminution of 2.5, which indicates 1.6 hydrogen.

These experiments were made in a tube of 0.4 inch internal diameter, divided into deci-millilitres, with such accuracy, that I could measure to within one quarter of a deci-millilitre, or the 650th of a cubic inch In a tube of such small diameter, a much longer time is requisite for perfect action than in a more capacious vessel, since in a jar of 1.3 inch diameter, the effect is quite complete in five minutes. In all such experiments as the preceding, where great nicety of observation is necessary, the thermometer was always observed, and due correction made for the least difference. The duration of the experiments was not considerable enough to render barometrical observations requisite, more particularly as I drew no inference from single experiments. I think it then fair to conclude, that hydrogen may be detected in atmospheric air by platinum, when it does not exceed $\frac{1}{100}$th of the whole volume; it follows too, if my experiments are not in fault, and I took all possible care to avoid error, that even the quantity of hydrogen under such circumstances is exactly indicated.

A mixture of 6 volumes of oxygen to 1 hydrogen was fired by electricity, and a platinum ball was brought in contact with the residue, but occasioned no diminution.

A mixture of 8 volumes of oxygen to 1 hydrogen was fired by electricity. The residue was not at all diminished by platinum.

A mixture of 9 volumes of oxygen to 1 of hydrogen was fired by electricity. The diminution was quite to the sufficient extent, nor had platinum any effect upon the residue.

In a mixture of 11 parts of oxygen with 1 of hydrogen, electricity occasioned only a feeble detonation, attended with partial decrease of volume, but a platinum ball caused formation of water in the residue.

A mixture of 13 oxygen to 1 hydrogen was acted on by electricity and platinum, just as in the preceding case.

A mixture of 15 volumes of oxygen and 1 hydrogen was electrified, without either detonation or decrease in volume. The ball No. 2. then caused a diminution, which indicated the exact quantity of hydrogen.

These results with respect to the influence of electricity correspond exactly with the experiments of MM. de Humboldt and Gay Lussac, detailed in the *Journal de Physique* for 1805.

I made various mixtures of oxygen and hydrogen, in which the latter was $\frac{1}{18}$th, $\frac{1}{20}$th, $\frac{1}{24}$th, $\frac{1}{30}$th, $\frac{1}{36}$th, of the whole mass, and in all these cases ball No. 2. caused a diminution, which indicated the quantity of hydrogen with much accuracy. In the two last experiments the ball was slightly warmed.

7 volumes of hydrogen were mixed with 700 volumes of oxygen, being the exact proportion of 1 : 100. The ball No. 2. gently warm, was put into 138 volumes of this mixture. The process was conducted in a capacious jar; the gases were carefully transferred into the graduated tube at the end of 10 minutes, and the diminution was 3 volumes. Hence 2 volumes of hydrogen were indicated, while 1.38 were present.

Ball No. 2. was put into 186 volumes of the same mixture, which was allowed to remain in the graduated tube for half an hour, when the diminution amounted to 4 volumes. 2.6 volumes of hydrogen were therefore indicated, while only 1.86 were present.

Though there is a very slight error of observation in both these experiments, we may nevertheless safely infer that hydrogen may be detected in oxygen gas, and its quantity ascertained with accuracy by platinum, when it does not exceed $\frac{1}{100}$th of the whole volume. To detect any unforeseen source of error, I performed the following experiment. No. 2. gently warm, was put into 187.5 volumes of atmospheric air, dried by fused potash, and was left there three-quarters of an hour; after this interval the air measured 189, which is 187.5 + 1.5 the space occupied by the ball. I had ascertained, by some preliminary trials, that platinum, though strongly heated, could not cause the combination of oxygen and nitrogen, as electricity does; and this is confirmed by the foregoing experiment. It shows, too,

that the atmospheric air contains either no hydrogen, or so very small a quantity as not to be rendered visible by the action of platinum.

Different portions of oxygen were added to hydrogen gas, and the electric spark passed through the mixtures. When 9 volumes of hydrogen were mixed with 1 of oxygen, electricity occasioned detonation; but when they were mixed in the proportion of 11 to 1, a strong charge from a Leyden-jar caused neither detonation nor diminution. Platinum, however, caused immediate formation of water, and the quantity of oxygen was indicated by the diminution.

I mixed 3 volumes of oxygen with 300 of hydrogen gas, with which a platinum-ball had been in contact for some hours. The ball No. 2, recently ignited, but quite cold, was put into 112 volumes of this mixture. In half an hour there was a diminution of 4 volumes, which indicates the presence of 1.3 oxygen, while 1.1 was the quantity present. The same ball was put into 154 volumes of the same mixture. In half an hour there was a diminution of 5.5 volumes, which indicates 1.8 oxygen, while 1.5 was the quantity present.

From these experiments, it is apparent that the presence of hydrogen gas in oxygen or atmospheric air, or of oxygen gas in hydrogen, may be detected, and the exact quantity of either indicated by the action of platinum. It is clear, likewise, that analytical chemistry has hitherto possessed no agent of equal delicacy for these gases, and, therefore, that platinum may be used with great advantage in eudiometry.

To put this conclusion to the test of experiment, I now proceeded to an analysis of atmospheric air. A jar full of the air of my apartment was dried over mercury, and deprived of carbonic acid by fused potash; known quantities of this air, mixed with hydrogen, were exposed to the action of an active platinum-ball.

1st Exp.	The result was	21.8 of Oxygen per cent.
2d Exp.		22.3
3d Exp.		21.7

The two first were performed in the narrow graduated tube, and lasted half an hour; the third was conducted in a capacious jar, and was over in five minutes.

The large quantity of oxygen indicated by these experiments surprised me much, and I was at a loss to conjecture whether this arose from the quantity of oxygen in the atmosphere being actually greater than was generally supposed, or whether there was not some source of error common to all the three experiments. It occurred to me, that the oxygen of the air contained in the hydrogen, as impurity, might perhaps be made to combine with a portion of hydrogen during the experiment, and thus occasion a greater diminution than there really ought to have been. It will be readily inferred from some preceding experiments that this did occur. I was not, however, aware of the circumstance on the present occasion, for this analysis was made prior to the experiments to which I allude *. Acting upon this supposition, I left an active ball during the night in contact with the hydrogen which I intended to use on the following day. I now made six experiments with great care, allowing the action to go on at one time in the graduated tube, at another in a capacious jar. In the latter case the diminution had attained its maximum in five or ten minutes, while half an hour was necessary in the former. The residue was dried by fused potash before being measured, and due correction made for change of temperature.

Exp. 1. indicated	20.3	per cent. of Oxygen.
Exp. 2.	20.3	
Exp. 3.	20.7	
Exp. 4.	21.0	
Exp. 5.	21.3	
Exp. 6.	21 7	

The mean of these six experiments is 20.88, so that we may safely assume 21 to be the correct number, which agrees very accurately with our best analyses upon this subject.

The action of platinum affords a neat and expeditious method of ascertaining the purity of hydrogen or oxygen. It is easy, too, to prepare nitrogen of great purity, by adding just sufficient hydrogen to combine with all the oxygen of a known quantity of air, and putting a platinum-ball into the mixture.

* To put this beyond a doubt, a platinum-ball was put into a known quantity of hydrogen. It did at first occasion a slight diminution of volume, proportionate to the quantity of air present, but afterwards had no farther action on the gas.

It appeared to me at a very early period of this investigation, that spongy platinum might be expected to produce the composition and decomposition of gases, whenever the electric spark succeeded in doing so. Professor Dœbereiner had himself found, that mixtures of carburetted hydrogen and carbonic oxide with oxygen gas were made to combine by this agent. MM. Dulong et Thenard have observed, that, in a mixture of hydrogen and nitrous gas, spongy platinum occasioned the decomposition of the latter, with formation of water and ammonia; and that it acted likewise on a mixture of hydrogen and nitrous oxide. My friend Mr Blundell, a most intelligent and promising student of this University, has likewise noticed some interesting facts of the same nature. He finds that platinum causes hydrogen to unite with chlorine, and with the elements of euchlorine gas; and has likewise observed other facts of a similar nature. In a few observations, read some weeks ago before the Royal Medical Society, I suggested the probability that the same agent would make iodine and hydrogen combine; and though I have not myself had leisure to examine this subject with care, yet Mr Blundell informs me he has succeeded in forming hydriodic acid in this way.

Several of these experiments I have myself repeated, and found perfectly correct. My attention, however, was chiefly directed to the action of platinum on mixtures of oxygen with olefiant gas, with coal gas, and with carbonic oxide. Notwithstanding the ingenious researches of Dr Henry, a method of separating hydrogen, light carburetted hydrogen, and carbonic oxide, from one another, is still a great desideratum in analytical chemistry; and I entertained some hope that platinum might prove useful in this point of view. My attempts to apply it to the analysis of these gases have failed, but as I have examined this subject with considerable care, it may not be uninteresting to relate some of the experiments.

A jet of coal-gas, procured from the city gas-pipes, was thrown upon freshly ignited spongy platinum. At the first moment a particle of the metal became luminous, but the light disappeared on the instant, nor have I been able, upon any subsequent occasion, to reproduce the same phenomenon, though platinum freshly reduced, and in a state of great activity, was

employed for the purpose. A jet of coal-gas and oxygen from separate vessels was thrown upon spongy platinum, without the least luminous appearance whatever; but if the metal be first strongly heated before the blowpipe, and a jet of coal-gas be thrown upon it immediately after all light has disappeared, the platinum quickly becomes red, and will continue so for any length of time. A vivid light is emitted, but the gas itself is not enflamed. This result would, *a priori*, be expected, because the heat necessary to inflame an explosive mixture of coal gas and atmospherical air, is greater than the white heat of solid bodies.

Coal-gas was mixed with rather more than twice its volume of oxygen. I heated a piece of spongy platinum to vivid redness; put it quickly on a little basin of platinum-foil, floating on the mercury, and covered it with a jar full of the explosive mixture. Watery vapour condensed on the surface of the mercury, the platinum became red, and rapid diminution followed: the redness, however, speedily disappeared, and the progress of the operation was arrested long before either gas was consumed. The platinum was left two hours after this in contact with the residue, but did not cause any marked reduction of its volume. The carbonic acid was absorbed by fused potash, and the remainder detonated strongly with the electric spark. This experiment was several times repeated with a similar result. It is curious that the process was in each case so quickly interrupted, for it is obvious that heat enough would be generated for its continuation. The cause is to be sought in the atmosphere of carbonic acid gas, which, as it forms, collects around the platinum, and thus prevents an adequate supply of the explosive mixture from reaching the metal, till its temperature has fallen below the point at which the combination can go on. I heated the balls No. 1. and 2. in a similar manner, and covered them with a jar full of the explosive mixture of coal-gas and oxygen. Rapid action ensued, without any visible redness; the process ceased when much of the mixture remained, and after the carbonic acid had been absorbed by potash, a spark of electricity exploded the residue.

A mixture of coal-gas and oxygen was divided into three portions. Pure spongy platinum was put into one portion, and

the balls No. 1. and 2. into the others, all being cold: no immediate action ensued, and the diminution after twelve hours was very slight; traces of carbonic acid were discoverable by lime-water, which shows that some chemical change had been occasioned. The experiment was repeated, with this difference only, that the mixtures were heated to near the boiling point of mercury. The diminution was now greater than before, and more carbonic acid had formed, but the residue was still explosive.

Coal-gas and hydrogen were mixed in various proportions, and to each mixture sufficient oxygen was added for complete combustion.

When the coal-gas was to the hydrogen as 3 : 1, No. 2. had no immediate action, at common temperatures; but if ignited before the blowpipe, and then plunged rapidly into the mixture, just after it had ceased to be red, an immediate diminution of volume followed, and the ball became luminous.

When the coal-gas and hydrogen were as 2 : 1, the ball No. 2. acted precisely as in the preceding experiment. The same occurred when the gases were as 3 : 2. When they were in the proportion of 2 : 3, and the ball was heated so as just to be borne on the hand, a rapid diminution of volume succeeded, without emission of light. The ball likewise acted, though with far less energy, when cold.

When the coal-gas and hydrogen were as 1 : 2, the ball acted nearly in the same manner as before, though with more energy.

In all these experiments there was copious production of carbonic acid, but the residue still contained an explosive mixture. The coal-gas, as obtained from the pipes, always contains some carbonic acid, which, of course, was previously removed by potash.

Olefiant gas, carefully prepared and well dried, was mixed with three times its volume of oxygen. Spongy platinum, as well as the balls, had hardly any action upon this mixture, when cold or gently warm. Heated to near the boiling point of mercury, a partial action succeeded, with production of carbonic acid. A platinum ball was heated to vivid redness before the blowpipe, and then introduced quickly into the mixture; it acted with energy, for there was on the instant a copious produc-

tion of water and carbonic acid, but still the residue contained some explosive mixture.

Olefiant gas and hydrogen were mixed in various proportions, and supplied with sufficient oxygen for complete combustion.

When the hydrogen bore a small proportion to the olefiant gas, a platinum-ball or the pure metal had no more effect than when no hydrogen was present.

When equal parts of these gases were present, a platinum ball had very little effect when cold, but if introduced when heated till it could he barely held in the hand, it caused a copious deposite of dew, with rapid diminution of volume. The residue was explosive.

A cold platinum-ball had very little effect when the olefiant gas was to the hydrogen as 1 : 2, but if gently warm it acted vigorously, and I have thus obtained even the entire consumption of the inflammable mixture, though this effect cannot be relied on.

A platinum-ball, strongly heated, was put into a similar mixture, containing olefiant gas and hydrogen in the proportion of 1 : 3. It caused a dense deposite of dew at the moment of its introduction, became red hot, and set fire to the mixture, which exploded with great violence.

Carbonic oxide gas was made the subject of similar experiments. A platinum-ball was put into a mixture composed of equal parts of this gas and oxygen, without any immediate effect: some diminution had taken place in the course of a few minutes, which was not much greater in twenty-four hours. A little carbonic acid had been formed, however, on the absorption of which, the residual gas was found strongly explosive. Pure spongy platinum acted in a similar manner. Balls No. 1. and 2. were brought in contact with different portions of a similar mixture, soon after they had ceased to be red hot. Rapid diminution succeeded, and carbonic acid was formed abundantly, but the residual gas was still explosive.

Spongy platinum was made red hot, and immediately after ceasing to be luminous, was covered by a jar of a similar mixture. The redness quickly reappeared, and the moment it did so, the gas exploded. Sir H. Davy has shown that a mixture of carbonic oxide and oxygen is inflamed by a heat considerably

lower than is required by an explosive mixture of oxygen and hydrogen gases, a fact with which the preceding experiment entirely corresponds.

Mixtures of carbonic oxide, hydrogen, and oxygen, were made in different proportions, and brought in contact with platinum at various temperatures. The results of these experiments, however, are so nearly analogous to those already related, that I need not give them in detail. Small quantities of hydrogen are not rendered obvious by the platinum; and the whole of an explosive mixture is not consumed, unless the hydrogen bear a large proportion to the carbonic oxide, and the ball be heated.

It appears from these experiments, that platinum can give us no aid in separating these different gases from one another. They shew, however, one very singular fact, namely, that great as the power of platinum is in effecting the combination of oxygen and hydrogen, the presence of certain gases has a very extraordinary influence, in diminishing that power. This observation has naturally led me to inquire into the causes which influence the action of platinum;— an inquiry intimately connected with the direct object of this paper, but which I have undertaken, not only on that account, but likewise with the design of drawing a parallel between platinum and electricity, relative to their action on explosive mixtures.

*(To be concluded in next Number *.)*

Art. IX.—*Notice of the Colossal Ray or Skate; with an Account of the Capture of one, at Port-Royal, Jamaica, where it is known under the name of Sea-Devil;* By A. Lamont, Esq. Lieutenant 91st Regiment.

Several of the species of the genus Raia are remarkable, on account of their vast size and strength. They occur principally in the Tropical Seas, one species only being known as an inhabitant of the European Seas, viz. the Ray of Fabroni, a na-

* We regret that want of room obliges us to delay, until next Number, the second part of this interesting Memoir of Dr Turner.

tive of the Mediterranean, which attains a breadth of 12 feet. Labat describes a monstrous skate, observed by the Negroes of Guadaloupe, as being 13 feet 8 inches broad, and about 10 feet from the snout to the insertion of the tail, which was 15 feet long, and 20 inches broad at its insertion, making the total length 25 feet. The flesh, he says, was not eaten by Europeans, but was salted by the Negroes. The monster named Manta, and said to suffocate the pearl-fishers, is probably the same, or a similar animal. The Raia Banksiana, found in the West Indian seas, Sir Joseph Banks informs us, is sometimes so large, that it requires seven pair of oxen to drag it along the ground. A species of Ray, probably the Banksiana, was lately killed on the coast of America, the capture of which is thus described by Mr Mitchell of New-York, in a letter to the President of the New-York Lyceum of Natural History.

"On the 9th day of September 1823, returned from a cruise off Delaware Bay the fishing smack Una. She had sailed about three weeks before from New-York, for the express purpose of catching an enormous fish, which had been reported to frequent the ocean a few leagues beyond Cape Henlopen. The adventurers in this bold enterprise have been successful. The creature is one of the huge individuals of the family of Raia, or, perhaps, may be erected, from its novelty and peculiarity, into a new genus, between the Squalus and the Acipenser. Its strength was such, that after the body had been penetrated by two strong and well-formed gigs of the best tempered iron, the shank of one of them was broken off, and the other singularly bent The boat containing the fishermen was connected, after the deadly instrument had taken hold, with the wounded inhabitant of the deep, by a strong warp or line. The celerity with which the fish swam could only be compared to that of the harpooned whale, dragging the boat after it with such speed, as to cause a wave to rise on each side of the furrow in which he moved, several feet higher than the boat itself. The weight of the fish after death was such, that three pair of oxen, one horse, and twenty-two men, all pulling together, with the surge of the Atlantic wave to help, could not convey it far to the dry beach. It was estimated from this (a probable estimate) to equal four tons and a half, or perhaps five tons. The

size was enormous; for the distance from the extremity of one wing or pectoral fin to the other, expanded like the wing of an eagle, measures 18 feet; over the extremity of the back, and, on the right line of the belly, 16 feet; the distance from the snout to the end of the tail, 14 feet; length of the tail 4 feet; width of the mouth 2 feet 9 inches. The operation of combat and killing lasted nine hours. The achievement was witnessed by crowds of citizens on the shores of New Jersey and Delaware, and by the persons on board the flotilla of vessels in the bay and offing. During the scuffle, the wings, side-flaps, or vast alated fins of the monster, lashed the sea with such vehemence, that the spray rose to the height of 30 feet, and rained around to the distance of 50 feet."

In the month of February last, the following interesting account of the capture of the colossal skate or ray was sent to me by my former pupil Lieutenant Lamont of the 91st Regiment.

"The first appearance of an animal of this species, since I have been here (about 18 months) was about two months ago, when I was called out to the beach by some of the inhabitants, whom I found, on going there, to be assembled in great numbers, to see what they called the *Sea Devil.* I confess my curiosity was not less excited than theirs, when I saw floating close to the surface of the water, about 20 yards from me, a large mass of living substance, of a dark colour, but of the shape and size of which I could not, at the time, form any proper idea, it being so very different from any thing I had ever before seen or heard of, farther than that I supposed it to have been many times the size of what I now believe it was. No time was lost in setting out in pursuit of him, with harpoons, &c.; and it was not long before he was come up with, and struck with one of the harpoons; when he made off with great velocity, towing the boat after him. As he seemed to incline chiefly to the surface of the water, 6 or 7 more harpoons were (with the assistance of several canoes that had come up) successively plunged into him, and all the boats made fast to each other, which he was obliged to pull after him, with several people in each. Such, however, was the great strength of this animal, that, after being fast, in the manner I have described, for upwards of 4 hours, and taking the boats out to sea attached to him, to a distance of about 10 miles from the

harbour, and having been pierced with so many wounds, he was still able to defy every effort to bring him in. It had now got late, and was dark, and an attempt was made to force him up near enough to get another large harpoon into him: this was no sooner done, than he darted off; and, by an almost unaccountable and seemingly convulsive effort, in a moment broke loose from all his fetters, carrying away with him 8 or 10 harpoons and pikes, and leaving every one staring at his neighbour in speechless astonishment, confounded at the power of an animal which could thus snatch himself from them at a time when they conceived him almost completely in their power.

"Since then, some of these animals have occasionally been heard of at a distance from the harbour; and, a few days ago, in coming over from Fort Augusta, with another gentleman, we fell in with one of them, which allowed us to get so near him that it was determined to set out the next morning to look for him. We did so; and took with us several large harpoons, muskets, pikes, &c. determined, if it were possible, to bring him in. He was descried, about 8 o'clock, near Greenwich, towards the top of the harbour, as usual floating near the surface, and moving slowly about. Having allowed the boat to get very close to him, he was struck with a harpoon, which was thrown at him in a most dexterous manner by Lieutenant St John, of the Royal Artillery. He immediately set out towards the mouth of the harbour, towing the boat after him with such velocity that it could not be overtaken by any of the others: after going in this way for near an hour, he turned back, which enabled the other boats to lay hold; and four of them were tied, one after the other, to the one in which he was harpooned, with four or five people in each of them. By this means we hoped to tire him out the sooner. In about an hour and a half after he was first struck, a favourable opportunity offering, a large five-pointed harpoon, made fast to a very heavy staff, was thrown at him with such an elevation that it should fall upon him with the whole weight of the weapon. This having been as well directed as the first, was lodged nearly in the middle of his back. The struggle he made at this time to get away was truly tremendous,—plunging in the midst of the boats,—darting from the bottom to the surface alternately,—dashing the water and

foam on every side of him,—and rolling round and round to extricate himself from the pole. This might be considered as having given him the *coup de grace*, although, at short intervals afterwards, he was struck with two more harpoons, and several musket-balls were fired into him. Still he was able to set out again, taking the four boats after him, which he carried along with the greatest ease. Having gone in this way for some time, he came to a stop, and laid himself to the bottom, when, with all the lines that were attached to him, it was quite impossible to move him. All expedients were nearly beginning to fail, when it was proposed to slacken the lines, which being done, had the desired effect, and he again set out. Having thus got him from the ground, inch by inch was gained upon him, till he was got near the surface, when he was struck with two large pikes. He now got rather faint; and the boats closing on him on every side, the combat became general with pikes, muskets, and every weapon we had. In fact, to such a pitch were all excited on the occasion, that, had a cool spectator seen the affray, he would undoubtedly have imagined that it was his *sable Majesty* himself that we had got amongst us. He was now towed ashore, being about 5 hours since he was first struck. This it required all the boats to do, and then but very slowly. His appearance now shewed the extraordinary tenacity of life of which this animal must be possessed, as his whole body was literally a heap of wounds, many of which were through and through, and he was not yet quite dead. This circumstance, with his great strength, is the cause of the name which has been given him by the fishermen here, as they have never been able to succeed in taking one of them, and were firmly of opinion that it was impossible to do so.

"This monster is of the flat-fish kind. On measurement, it was found to be in length and breadth nearly the same, about fifteen feet, and in depth from three to four feet. It had the appearance of having no head, as there was no prominence at its mouth: on the contrary, its exterior margin formed, as it were, the segment of a circle, with its arc towards the animal's body, and opening into a large cavity of about two feet and a half in width, without teeth, into which a man went with so much ease, that I do not exaggerate when I say, that another might have

done so at the same time. On each side of the mouth projected a mass of cartilaginous substance like horns, about a foot and a half long, and capable of meeting before the mouth. These feelers moved about a great deal in swimming, and are probably of use in feeding. On looking on this animal as it lay on the ground with its back upwards, it might be said to be nearly of equal dimensions on every side, with the exception of the two lateral extremities, extending to a point about four feet from the body, and a tail about five feet long, four and a half inches in diameter at the root, and tapering to a point. Above the root of the tail was the dorsal fin, and on each side of it a flat and flabby substance lying close to the body, of the appearance of fins. There were no other distinct fins, and its sole propelling power seemed to be its two lateral extremities, which become very flat and thin towards the point. As it shows these much in swimming, it gives a spectator an extraordinary idea of its size, as, when imperfectly seen, the conclusion naturally is, if the breadth is so great, how much greater must the length be. This animal was a female, and viviparous. On opening it, a young one, about 20 pounds weight, was taken out, perfectly formed, and which has been preserved. Wishing to know what it fed upon, I saw the stomach opened, which was round, about eight inches in diameter, and quite empty. It was closely studded over with circular spots of a muscular substance. Under the stomach was a long bag, with transverse muscular layers from end to end, and which contained nothing but some slime and gravel. This muscular appearance of the digestive organs would lead one to suppose, that it fed upon other fish, as is the general opinion here, though its having no teeth does not support that idea. Its weight was so great that it was impossible to ascertain it at the time; but some idea may be formed of it, when I assure you, that it was with difficulty that forty men, with two lines attached to it, could drag it along the ground. Its bones were soft, and, with the exception of the jaw-bones, could be cut with a knife. One ridge of bone ran from the mouth to the middle of the back, where it was met by another running transversely, from the extremities of which there were two larger ones converging towards the tail."

ART. X.—*Mean of Twelve Months' Meteorological Observations in the years* 1822–3, *made at Paramatta, New South Wales.* By His Excellency Sir THOMAS BRISBANE, K.C.B. F.R.S. M.W.S. &c.

	THERMOMETER.		BAROMETER.		HYGROMETER.		RAIN.
1822, May, .	highest,	72°	highest,	30,23			
	lowest,	42	lowest,	29,50			
	mean,	60	mean,	29,87			
June, .	highest,	67	highest,	30,20			
	lowest,	26	lowest,	29,55			
	mean,	53,3	mean,	30,09			
July, .	highest,	63	highest,	30,25			
	lowest,	27	lowest,	29,54			
	mean,	51,5	mean,	29,96			
August,	highest,	77	highest,	30,20			
	lowest,	35	lowest,	29,54			
	mean,	56,5	mean,	29,80			
October,	highest,	93	highest,	30,20	highest,	60	Inches.
	lowest,	36	lowest,	29,70	lowest,	26	3,413
	mean,	62	mean,	29,92	mean,	44,5	
November,	highest,	106	highest,	30,23	highest,	87	
	lowest,	42	lowest,	29,72	lowest,	40	0,516
	mean,	68	mean,	30,02	mean,	51,5	
December,	highest,	99	highest,	30,35	highest,	88	
	lowest,	48	lowest,	29,53	lowest,	24	5,235
	mean,	72	mean,	29,88	mean,	50	
1823, January,	highest,	102	highest,	30,30	highest,	77	
	lowest,	47	lowest,	29,60	lowest,	35	1,092
	mean,	74	mean,	30,05	mean,	53,5	
February,	highest,	106	highest,	30,30	highest,	100	
	lowest,	52	lowest,	29,78	lowest,	24	5,261
	mean,	73	mean,	30,12	mean,	49	
March, .	highest,	97	highest,	30,25	highest,	78	
	lowest,	49,5	lowest,	29,62	lowest,	24	6,660
	mean,	68,5	mean,	29,91	mean,	40	
April, .	highest,	98	highest,	30,45	highest,	48	
	lowest,	49,5	lowest,	29,77	lowest,	12	7,215
	mean,	60	mean,	30,15	mean,	33	
May, .	highest,	74	highest,	30,44	highest,	74	
	lowest,	40	lowest,	29,60	lowest,	22	0,556
	mean,	59	mean,	30,05	mean,	44	

ό (Zero) of the Hygrometer the greatest damp; 100° the greatest drought; elevation of Barometer 62 feet above the level of the sea.

ART. XI.—*Memoir on several Masses of Iron found in the Eastern Cordillera of the Andes.* By MARIANO DE RIVERO of Santa Fé de Bogota, and J. B. BOUSSINGAULT. Communicated by the Authors to Mr HEULAND. (Pl. IV.)

ON our arrival at Santa Rosa, a village on the road from Pamplona to Bogota, we were told that an iron mine had been discovered in its vicinity, and that a specimen of the mineral was serving as an anvil to a blacksmith. On inspection, we were agreeably surprised to find that this specimen possessed all the characters of meteoric iron. This mass of iron was found upon the hill of Tocavita, at the distance of a quarter of a Spanish league to the east of the village, on Easter Saturday of the year 1810, by Mr Cecilia Corredor, a native of the said place. We went to examine the hill, where we still saw the cavity which was made at the time the mass was taken out, and which, when found, was nearly buried, and only visible to the extent of a few inches. The formation of the hill of Tocavita belongs, like that of Santa Rosa, to a secondary sandstone, and is observed to a considerable extent. The latitude of Santa Rosa is 5° 40′ N., and the longitude 75° 40′ W. of Paris; its height above the level of the sea is 2744 metres.

The inhabitants joined in getting this mass to the village, where it was deposited at the town-house for seven years; and seven years after that, till our arrival, it was used by the blacksmith. This iron contains cavities, but we have found no vitrified traces in them; it is malleable, and can be cut; its structure is granular, its lustre of a silver-white, and the specific gravity 7.3. The weight of this mass was much exaggerated by our informants; but, judging from its diameter, notwithstanding the irregularity of the surface, it will be found that its volume is nearly 102 cubic decimetres, and consequently its weight must approach 75 myriogrammes. (One myriogramme is the weight of 21 lb. 11 ounces 12 adarmes and 3 grains Spanish.)

A fact worthy of notice is, that, nearly at the same time when this mass was found, other pieces were met with on the same

* Translated from the Spanish.

MASSES OF METEORIC IRON.

This mass was found on the Oriental Cordillera of the Andes of New Granada near Santa Rosa, weight about 15 Cwt.

Found at Rasgata in the Cordillera of Zipaquira weight 30 pounds

From Santa Rosa, weight 1 lb. 4 oz.

Published by A. Constable & Co. Edinr. 1834

hill, and in the neighbourhood of Santa Rosa, in various directions. It seems they are scattered about abundantly, as, during our short stay there, we fell in with a considerable number of them. To convince ourselves of the identity of this iron, with such as other travellers describe, we had but to examine it chemically, and the following are the details of our analysis.

Analysis of the mass of Iron weighing 75 myriogrammes.

1.28 grains were dissolved in nitric acid. The solution was rapid, and left but a small residue, evaporated almost to dryness, so as to oxidise the iron; water was added afterwards, to precipitate it with ammonia; and the precipitated oxide of iron was separated, filtered, and washed in hot water. The ammonial liquor was sufficiently tinged with green and azure colours. With the prussiate of potash it precipitates abundantly, and is of a white slightly greenish colour, indicating the ammonia to have been coloured by nickel, and not by copper. To this solution of ammonia, after half its quantity was evaporated, caustic potash was added, and, to make certain of the entire decomposition of the double salts of ammonia and nickel, the evaporation was carried on to dryness; to the residue, water was added; and the oxide of nickel procured, after being washed and calcined, weighed 0.14. To find the nickel that might have remained with the oxide of iron, precipitated by the nitric solution, this oxide, still moist, was dissolved in the pyro-ligneous acid; the solution was evaporated, and the residue, tried with the proper cautions, was filtered, and to the filtered liquor carbonate of potash was added, forming a precipitate slightly white; it was boiled, and calcined, weighing 0.01, and proved to be oxide of nickel. No traces of cobalt or manganese could be discovered in the smallest possible quantity; and the results were, oxide of iron, 1.17; oxide of nickel, 0.15.

100 parts gave,	Iron,	91.51
————	Nickel,	8.59

We proceeded in the same manner with the other specimens, and give herewith the results. And first, of the mass, weighing 681 grammes (*gramos*), found in 1810 near Santa Rosa. This

iron is malleable, hard to cut, its lustre is silver-white, its grain as fine as that of steel, forges rather indifferently, and is brittle when heated; its specific gravity is 7.6.

7.18 grains gave Oxide of Iron,		9.46
Oxide of Nickel,		0.75
Residue insoluble in Nitric Acid,		0.02
100 parts gave, Iron, -	91.23	
Nickel, -	8.21	
Residue, -	0.28	

The residue is insoluble in the nitric acid, and is hardly acted upon by the nitro-muriatic acid even in a state of ebullition. It seems to be composed of nickel and iron, and perhaps may contain a little chrome.

Another mass, weighing 561 grammes, also found near Santa Rosa in the year 1810, is of a porous structure, malleable, very hard to file, with the metallic silver lustre, and its grain similar to that of fused steel.

1.98 grains gave Oxide of Iron,		2.62
Oxide of Nickel,		0.16
100 parts, Iron, - -	91.76	
Nickel, - -	6.36	

We have traced the nickel in many other specimens, which were found with the preceding about the same time, in the vicinity of Santa Rosa; the largest weighed 145 grammes. But it is not only in the neighbourhood of Santa Rosa that iron has been met with in a metallic state. We shall therefore still mention two masses, which were discovered at some distance from the salt-mine of Zipaquirá, at a spot named Rasgatá. The one belongs to Mr Geronimus Torres; it weighs 41 kiliogrammes, is without traces of cavities, very malleable, of a structure composed of very minute planes, yielding with difficulty to the file, of a metallic lustre, and having its specific gravity = 7.6.

4 grains gave Oxide of Iron,		5.23
Oxide of Nickel,		0.40
100 parts gave Iron,	90.76	
Nickel, -	7.87	

The other mass weighs 22 kiliogrammes, is very porous, almost spheroidal, a very malleable iron, of a foliated texture, and its silvery lustre gives it the aspect of certain irons, glossed with

white. When trying to find nickel, it seemed to contain 7 to 8 per cent.

Zipaquirà, in the limits of which these last masses were found, is in Lat. 4° 57′ N., and Long. 76° 33′ W. of Paris. Its elevation above the level of the sea is 2650 metres *.

ART. XII.—*Abstract of a Report on the State of the Elm Trees in St James's and Hyde Parks.* Drawn up at the request of Lord Sidney, the Ranger, for the Treasury, by W. S. MACLEAY, Esq. A. M. F. L. S. &c. &c.

[The following remarks compose the substance of a Report on the State of the Elm Trees in St James's and Hyde Parks, which was lately drawn up at the request of Lord Sidney the Ranger, in order to be presented to the Treasury. As it is possible that some of your readers may have trees affected in a similar manner, perhaps a communication on the subject may not be deemed unworthy of a place in the pages of the Edinburgh Philosophical Journal. I have the honour to be, &c.

To Professor JAMESON. W. S. MACLEAY.]

SO little attention has hitherto been paid to the causes of disease in trees, that few persons ever think of attributing it to any other origin than one entirely vegetable, or, in other words, to the constitution of the plant itself. Yet, in every case, perhaps, where the disease is infectious, and particularly where it is confined in a plantation or forest to the individuals of one species of tree, we may reckon with certainty on its proceeding from the attacks of some insect. Every tree, nay, indeed, every plant seems to have one or more species of insect destined by Nature to feed on it; and when, from the combination of various causes (such, for instance, as the weakness of vegetation in a particular air or soil, inattention to the evil at the proper time for effectually checking it, &c.), the number of insects which attack trees

* Mr Rivero, in his original memoir, chooses to name these masses 'Problematical Irons,' at the same time proving them to be meteorical; and undoubtedly they differ in no essential respect from those we have seen, and possess, from Tucuman, in the province of Buenos Ayres, from Mexico, Brazil, and many other places.—*Heuland.*

becomes increased beyond its due limit, we must either apply the axe without scruple to the seat of the disease, or make up our minds to submit to the utter destruction of our plantations.

Almost all timber-eating insects are comprised in three orders, viz. *Coleoptera*, or beetles; *Lepidoptera*, or moths, butterflies, &c.; and *Hymenoptera*, or bees, wasps, &c. All these in their youngest state are worms or larvæ, and it is while in this stage of their life that they commit the direct injury to the trees, either by gnawing off the bark, or by devouring the wood. The communication of the disease to other trees is periodical; for when the above mentioned worms or larvæ arrive at their perfect and winged state, the mischief committed by them *directly* is comparatively trifling, and, in fact, generally results, not so much from their voracity, as from their attempts to extricate themselves, and to arrive at the external air, or from their endeavours to commit their eggs to a proper nidus. But as they are now winged, and capable of depositing myriads of eggs, the germs of as many devouring larvæ, the disease is thus dispersed throughout the neighbourhood of the tree originally infected. If, however, it be, in this their perfect state, that the insects are most formidable, having attained the power of propagating the disease, it is also from an accurate knowledge of them while in this state, that we can alone derive any hope of being able to counteract their mischief.

The first thing, indeed, to be done in all such cases is, to ascertain the species of perfect insect which occasions the disease. The experienced naturalist is able from this examination of the worm or larva which he finds devouring the wood, not only to ascertain the order and family, but often the genus, and even the species of winged insect which has produced it; and having determined the genus or species, it becomes an easy matter to know the season of its appearance. None of these timber-eating insects remain in their perfect or winged state throughout the year, and rarely for more than eight weeks. We may therefore easily ascertain the proper time for cutting down those trees which are so much infected with larvæ as to afford no hope of saving them; for it would obviously be the height of imprudence not to seize the only opportunity of preventing the annual

dispersion of the disease, by destroying the brood of larvæ while yet in the tree.

As soon as metamorphosis has taken place, as soon as the winged insect has made its appearance, the mischief for the ensuing year is done. Not only is time thus lost, and more trees inevitably destroyed, but the future eradication of the disease is rendered much more difficult.

We may also derive another advantage from ascertaining the habits of that particular insect which causes the disease, and the season of its appearance; for it is often thus possible, by timely measures, even in places where the perfect insect is prevalent, to prevent any deposition of eggs in such wood as may have remained sound. At all events, the devastation committed by these animals is at times so great, that it is clearly worth while to make experiments to obviate it; although it is difficult to conceive how such experiments can ever be made philosophically by persons who do not in the first instance make themselves acquainted with the natural history of that particular species of destructive insect which may have occasioned the mischief.

Of the evil which is mentioned above in general terms, St James's and Hyde Parks afford us at present too many examples. The elm-trees in both, and particularly in St James's Park, are rapidly disappearing, and unless decisive measures be soon taken to resist the progress of the contagion, we must not only expect every tree of this species to be destroyed in the Parks, but may have to regret the dissemination of the evil throughout the vicinity of London.

In St James's Park, which has more particularly been subject to my examination, there are several species of beetle to be found attacking the elms. That species, however, which occasions all the havoc which we have now to lament in the Mall and Bird-Cage Walk, is the *Hylesinus destructor* of Fabricius, or *Scolytus destructor* of Latreille, an insect of which the history is briefly as follows.

A small beetle, with its head rather covered with hair, having a polished black thorax, and brown wing-cases, may be seen in numbers running over the trunks of the elms from the end of March to the first days of July, but principally about the end

of May or commencement of June. It may then be seen entering into holes, with which the bark appears perforated as though with a gimlet. It insinuates itself into these holes, or into the crevices of the bark, for the purpose of depositing its eggs. On stripping off a piece of the loose bark, we may easily at any season understand how the barking of trees is effected by these minute animals, for the surface of the wood thus exposed presents to the view innumerable impressions, which may be compared to impressions or casts of large and broad scolopendræ.

The middle or body of this singular impression * marks the path of the perfect female insect, while employed in laying her eggs, which is to her, as to most other winged insects, the immediate forerunner of death. From this tubular path, however, in which she deposits her eggs, the larvæ, which are hatched from these eggs, in the shape of little white apod worms, proceed nearly at right angles, eating their way in parallel smaller tubes, which, lying close to each other, effectually serve to separate the bark from the tree. The larvæ remain feeding in the tree, generally between the bark and the wood, throughout the winter season. About the commencement of spring they assume the pupa or nymph state, and, before the end of this season, the bark of an infected tree begins to appear as if all its crevices were full of a very fine saw-dust. The last change of the insect takes place; and being now winged, it tries to arrive at the external air, for the purpose of propagating its species, and laying its eggs in other trees. Each hole which now appears as if made with a gimlet, marks the exit of a perfect insect. In the first instance, the voracity of the larvæ, and, in the second, the endeavours of the perfect insects to liberate themselves from the wood, particularly when such attempts are made by almost infinite numbers, soon occasion the bark to fall in large pieces. The consequence is, that the new leaves only make their appearance to wither, and the tree perishes.

The early entomologists, little acquainted with metamor-

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* Impressions of a similar nature, when made on the wood by insects of a neighbouring genus, have given rise to such trivial names as *typographus*, *micrographus*, &c. with which the entomologist is so familiar in the genus *Bostrichus*.

phosis, on finding the perfect *Hylesinus destructor (le Scolyte* of Geoffroy), on dead or dying trees, erroneously considered their disease to be the cause, and not to be the effect of the insect's appearance. Hence the *habitat* of this species, in many of the older entomological works is said to be *rotten wood.* The absurdity of this notion will, however, be obvious, on the slightest investigation of the evil, in its earlier stages. It is, for instance, allowed on all hands, that the tree perishes by being barked; and the mere inspection of any of the trees so barked, will sufficiently shew that the mischief is effected in the manner above stated. In order to prove that experienced naturalists are now aware of the true cause of such vegetable diseases, it may be sufficient for me to cite, from the third volume of M. Cuvier's Regne Animal, the prefatory remarks of M. Latreille on the whole family of insects to which the *Hylesinus destructor* belongs: "Les *Xylophages* vivent presque tous dans le bois; les larves percent ou y creusent des sillons en divers sens; et lorsqu'elles sont très abondantes dans une forêt, particulierement dans celles de pins et de sapins, elles font périr en peu d'années, une grande quantité d'arbres, ou les mettent hors d'état d'etre employés utilement dans les arts."

On a review of the above remarks, it seems, in the first place, advisable for those persons who have elm trees in the state of those in the Parks, to inspect them twice every year, first, in summer, when the perfect insect is on wing; and, secondly, in winter, when those trees which are much infected ought to be cut down. Such trees ought, if possible, to be burned with the larvæ in them, or if this be not convenient, they should immediately be subjected to such heat or fumigations as may destroy the larvæ, which at this season are near the surface, and therefore, not so difficult to kill. To rest content with having cut down the trees, without destroying the larvæ, or even removing the trunks from the vicinity of the sound wood, is, in reality, to do no good at all.

It may also be recommended, that no more elm-trees be, during the continuation of the disease, planted in places where it is prevalent. The *Hylesinus destructor* is peculiar to the elm, and was in all probability introduced into the Parks, with some

of the young elms which have lately been planted in different parts of them. The other species of trees, in these places of public resort, seem, on the whole, to be very free from disease; but in planting, and particularly in ornamental planting, it may be well to bear in mind, that indigenous trees are much more subject to the attacks of our indigenous insects, than those which are not natives of this country.

With respect to those trees which are in an unsound state, it is very difficult to point out a cure for them. When the insects attack the branches, these ought obviously to be examined, and if infected, ought, as soon as possible, to be lopped off, and burnt. I scarcely know, however, what to propose, for the preservation of those trees, of which the trunks are infected. Perhaps it may be of use to cover over, in the month of March, with a mixture of tar and train oil, to a certain height from the ground, all such trees as it may be thought proper to attempt to save. I venture to recommend this coating of tar, not only by way of experiment, as protecting the trunks from the access of the perfect insects, but for the purpose of filling those little round holes, which, it is easy in summer for an accurate observer to perceive, afford peculiar facilities for the communication of the disease.

To those persons who, being unacquainted with natural history, may therefore be disposed to neglect the power of these insects, because they are individually minute, and who totally overlook the consequences of their being almost infinite in number, I have only to remark, that they may judge from what these animals have already done in the Parks, how much mischief they are capable of effecting. In the year 1780, an insect *, of the same natural family as the *Hylesinus destructor*, made its appearance in the pine-forests of the Hartz, and was neglected. In the year 1783, whole forests had disappeared, and, for want of fuel, an end was nearly put to the mining operations of that extensive range of country. At the present moment, also, the French Government is in alarm at the devastation committed in their arsenals, by an insect well known to naturalists, under the name of *Lymexylon navale*. About ten years ago, the principal naval engineer at Toulon, M. de Ceri-

* *Bostrichus typographus*, Fab.

sier, who happened to be conversant with entomology, discovered this insect in the dock-yards, and recommended certain precautions to be taken for the preservation of the timber there lodged. The French Government objected to the expence requisite for obviating an evil, of which, as yet, they had no experience; and now, when perhaps it is too late, the minister of Marine has determined to follow M. de Cerisier's advice. It is from such instances that we perceive the truth of an observation made by a French academician, while alluding to the devastation which such insects may occasion: "L'histoire de ces animaux merite d'être connu, à raison de son extrême importance, de tous les grands proprietaires et surtout par les inspecteurs généraux de nos forêts; elles ont aussi leurs insectes destructeurs, et ils verroient combien de causes qui dans le principe ne fixent au moment l'attention peuvent par négligence devenir funestes à l'état."

April 1824.

ART. XIII.—*Analysis of Cinnamon-stone from Ceylon.* By Dr C. G. GMELIN of Tübingen. Communicated by the Author.

THE composition of this mineral has afforded a subject of inquiry to Lampadius and Klaproth. The former thought he had detected in it a considerable quantity of zirconia and potash; but the analysis was conducted in so defective a manner, and the results are so contradictory to the first principles of chemistry, that it is impossible to believe the investigation had been conducted by Lampadius himself. Klaproth, whose name will never become extinct in chemistry and mineralogy, found the following results: Silica, 38.80; Lime, 31.25; Alumina, 21.20; Oxide of Iron, 6.50; loss, 2.25; (*Beiträge*, vol. v. p. 142.) It was proved by this analysis, that Cinnamon-stone is very nearly allied in composition to Garnet; and Professor Mohs has joined it to that genus under the name of Prismatic Garnet. Through the liberality of Mr Heuland, I obtained a specimen of cinnamon-stone, the genuineness of which is placed beyond all doubt, by its possessing all the external characters of that mineral,

Small particles of a green colour were imbedded in it, which were found to consist of phosphate of lime, with some carbonate of lime.

The phenomena of cinnamon-stone, subjected to the action of the blowpipe, were found to be the same as those described by Berzelius in his treatise on that instrument. Its specific gravity was = 3·783; temp. + 10 R.; 0.778 grains were only employed to ascertain it, and hence it is probably too high.

a. 1.535 grains of cinnamon-stone, in coarse pieces, left, after ignition, 1.530 gr.: 100 parts therefore would lose 0.326.

b. 3.117 grains of the mineral very finely powdered, were mixed with five times its weight of carbonate of barytes, and ignited. The mass after ignition was loosely coherent, and of a greenish grey colour. It was dissolved in muriatic acid, and the solution evaporated to dryness in a water-bath, by a very gentle heat. The silica weighed after ignition 1.247 grains, = 40.006 per cent.

c. The liquor was then diluted with a large quantity of water, the barytes removed by sulphuric acid, and the sulphate of barytes well washed upon the filter with boiling water. The liquor was now concentrated by evaporation, by which gypsum was separated, which, after being washed upon the filter with cold water, and ignited, weighed 1.945 gr. = 25.919 per cent. of lime. This gypsum was converted into carbonate of lime, by boiling it with a solution of subcarbonate of potash. The carbonate was entirely dissolved in muriatic acid with effervescence, and the salt thus formed had all the characters of muriate of lime.

d. The gypsum having been removed, the liquor was precipitated by caustic ammonia, and quickly filtered. The lime still contained in it was thrown down by oxalate of ammonia. The oxalate of lime afforded by ignition 0.223 gr. of carbonate of lime = 4.034 per cent. of lime.

e. The liquor was then evaporated, and the residue ignited. There remained a fused salt weighing 0.034 gr. of carbonate of lime = 4.034 gr. It was precipitated by muriate of platina, and was therefore sulphate of potash. These 0.034 gr. indicate 0.589 per cent. of potash.

f. The precipitate thrown down by caustic ammonia was dissolved in muriatic acid, and digested with a considerable ex-

cess of carbonate of ammonia. The liquor was separated by the filter from the undissolved part, and evaporated. A white substance fell down, which, after ignition, weighed 0.0463 gr. = 1.485 per cent. Considering the manner in which this subtance had been obtained, one might have taken it for Glucine, Ittria, or Zircon, but it was pure Alumina; and when dissolved in sulphuric acid by digestion, and mixed with a little ammonia, it crystallized entirely into alum. It follows, hence, that when alumina is precipitated by means of carbonate of ammonia, it is not advisable to add the latter in great excess; and that the common method used to separate glucine from alumina does not yield it perfectly pure.

What had been left undissolved in *f*, was dissolved in muriatic acid, evaporated in order to drive off the excess of acid, and then boiled with caustic potash in excess. The alkaline solution was separated from the brown-precipitate by the filter, and the alumina contained in it thrown down in the ordinary manner. It weighed after ignition 0.6705 gr. = 21. 511 per cent.

n. The brown residue in *g* was dissolved in muriatic acid, the solution boiled with nitric acid, and the iron now precipitated by succinate of ammonia. There were obtained 0.1143 gr. of oxide of iron, = 3.666 per cent.

i. The liquor remaining after the separation of iron, was now boiled with a solution of sub-carbonate of potash. A precipitate fell down, which, after ignition, weighed 0.0343 gr., and which was found to be lime with some traces of manganese. It must, therefore, be accounted for 0.620 per cent. of lime.

The cinnamon-stone is accordingly composed of,

			Oxygen.
Silica, - - -		40.006	20.12
Alumina,	1.485 (*f*)		
	21.511 (*g*)	22.996	10.74
Lime,	25.919 (*c*)		
	4.034 (*d*)		
	0.620 (*i*)	30.573	8.59
Oxide of Iron, - -		3.666 (*h*)	1.11
Potash, - - -		0.589 (*e*)	
Trace of Manganese.			
Volatile matter, - -		0.326 (*a*)	
		98.156	

It is obvious, that the oxygen of the silica is equal to the oxygen of all the bases together. Professor Berzelius has given the following formula for the composition of cinnamon-stone, according to the analysis of Klaproth: S F S + 4 C S + S A S. The formula according to the preceding analysis would be: F S + 8 C S + 10 A S. But when it is observed that the colour of this mineral is a more or less dark one, and that it contains of course a larger or a smaller quantity of iron, it may be inferred that it cannot be decided with any precision, how far the iron ought to be considered as an essential basis, or only as a colouring matter; and it is obvious, that a change taking place in the quantity of iron, would change at the same time the whole formula. In such a case, therefore, no weight can, I think, be put upon the real signification of such a formula.

The correctness of the statement of Klaproth respecting the composition of cinnamon-stone, is fully confirmed by the above analysis, conducted in a quite different manner.

Art. XIV.—*Notice regarding the Island of Grimsey, off the North Coast of Iceland, and the Isles of St Kilda, on the North-West Coast of Scotland.*

1. *Island of Grimsey.*

In the "Magazine des Voyages" of Nyerup, vol. iii. p. 312., there is a short account of an excursion in a fishing-boat from the north coast of Iceland to a small inhabited island about nineteen leagues from land. Mention is made of this island by its Danish name Grimsöe, in Olafsen and Povelsen's Travels in Iceland, vol. ii. p. 624., and in those of Olavius, vol. ii. p. 230.; but these travellers have only spoken of it in a cursory manner, and it appears that no person had visited it from Iceland in the memory of any of the present inhabitants. It lies to the north of Fliot-Horn, one of the northern capes of Iceland, and is about twelve leagues within the polar circle.

Mr Faber, the author of the narrative, who is known by a work on Ornithology, visited this island, with the object of examining the sea-birds which resort to it in summer to breed. He

set out from Oëfiord toward the end of May 1820, in a small six-oared fishing-boat, the crew of which were in quest of Hakalls (Squalus Carcharias). When about three leagues from the island, they saw numerous flocks of birds swimming around the boat. They landed on the 28th of May, at which time the island was still covered with a thick bed of snow, which only began to melt in the beginning of June. There were on this melancholy rock eight miserable earthen huts, the inhabitants of which, to the number of fifty, subsist chiefly by fishing. They are extremely poor, and are even exposed to all the horrors of famine when the winter is unusually prolonged, and the floating ice from Greenland keeps them blocked up, so as to deprive them of the resources which the fishery presents them. In the time of Olavius, (from 1770 to 1780), they had 3 cows, 80 sheep, and a horse, which being 30 years old, he observes, was regarded by them as a sort of antiquity. When Mr Faber visited the island, he found their stock reduced to a few sheep.

A principal part of the food of these poor people is derived from the birds which cover the cliffs of the island. These are of the usual species which frequent the polar seas; *Uria Troile*, *Uria Alle*, *Alca Torda*, *Procellaria glacialis*, several species of *Carbo* and *Larus*, and, above all, an incredible multitude of *Larus tridactylus*. Mr Faber observes of the *Uria Alle*, that this pretty little bird, so abundant in the northern seas, secures its eggs, by depositing them in the crevices of rocks, and among the fragments which have fallen from the cliffs; in this respect resembling the Uria Grylle, so common on many parts of our own coasts, while the other species of the genus deposite their solitary egg upon an exposed shelf of the rock, without any nest.

Bird-catching was an art in which the inhabitants were very expert. They had procured thongs of hides, which they used for ropes in descending the precipices. It may here be observed, that the preference given by the rock-men of our own islands to ropes, composed of twisted thongs or of hair, over those made of hemp, arises from the latter being more liable to cut. The danger accompanying this pursuit may readily be imagined, and the person who went down, had for himself a double share of the plunder. During Mr Faber's stay on the island, a

young man perished in this occupation, a fragment of rock which had been detached, having fallen upon his head. The body of this unfortunate person was deposited, according to custom, in the church. It was here where Mr Faber lodged, and the presence of such a companion rendered still more disagreeable his residence in this cold and damp place. He accordingly determined to return to Iceland on the 20th June; but not without having previously obtained specimens of the different birds which breed upon the island.

It is to be regretted, that the author has not given more details regarding this island, interesting as it is, both as the abode of human beings, and as the resort of so many species of birds, more especially, as there is little probability of its being soon visited again by any intelligent traveller. With regard to the latter subject, we cannot supply the deficiency from imagination, as it is wholly excluded from the circuit of physical science; but in regard to the former, we may be permitted to indulge in contemplating the hard lot of so many fellow creatures. We can picture to ourselves a rock in the polar seas, covered with a deep coating of snow and ice, washed over by the spray of a tempestuous ocean, enveloped in the gloom of tumultuous clouds; or we can fancy it surrounded by masses of ice, on which the polar bear, which is sometimes carried even to the coasts of Iceland, has come to spread terror among the inhabitants. We can feel part of the misery which famine produces, in a country where the resources are so few,—where the occasional protraction of the cold season has exhausted the scanty store of provisions, and delayed the arrival of the birds; but we cannot enter into the details of the picture, for we have not experienced the cold reality of scenes where human existence has verged upon the extreme limits of animal life. On the other hand, we rejoice to feel that there are scenes of joy even here; that the season of hope returns, when the sun-beam dances upon the blue waters around, and the air is filled with the mingling screams of the sea-birds, which have now revisited their usual haunts, when the ocean teems with fish, and the cliffs are covered with eggs. Even in Grimsey, bleak and desolate, and secluded as it is, contentment is not a stranger; and the warmest emotions of the heart may be equally developed and cherished

in it, as in those favoured climes, where the fancy delights to roam. Hospitality is a virtue common to all the northern tribes, more especially in the ruder states of society; and a belief in the protection of Providence, coupled with resignation to its awards, was found to be not less characteristic of the natives of Grimsey than of their Iceland neighbours.

2. *Isles of St Kilda.*

In Britain we have usually been led to imagine the lot of the inhabitants of St Kilda as peculiarly calculated to excite feelings of commiseration, and, indeed, in certain points of view, it is so; but the privations of these people are so little comparable to those of the inhabitants of Grimsey, that they may even, in many respects, afford a striking contrast. We have been accustomed to picture to ourselves—a solitary island in the Atlantic, the resort of sea-birds, surrounded by a tempestuous ocean, swept over by the cold blasts of the north, and drenched with the western rains, affording a hard-earned subsistence to a set of miserable wretches, wild as the birds of their native rocks, destitute of every comfort of life, and even barely supplied with its necessaries. But this representation is more poetical than true to nature; and though some of its features are in a certain degree correct, those which regard the condition of the inhabitants of St Kilda are much overstrained. It would be needless to institute a methodical examination of the subject; but it may be useful here to present a short account of the remote isles of St Kilda, which have at all times been considered as highly interesting, and to which, in this part of the country, the attention of the public has of late been more particularly directed.

The Isles of St Kilda lie at a distance of about 50 miles to the NW. of the middle division of the Outer Hebrides, from which they are easily seen in clear weather, owing to their great height. There are two principal islands, Hirt and Boreray, situate about 10 or 12 miles apart, and several smaller islands, or rocks, attached to them. Hirt is the only inhabited island, Boreray being too rugged and bare for the abode of man. These islands are referred to the Trap formation. Hirt, which is nearly three miles in diameter, rises to the height of about 2000 feet, or more; and Boreray is little inferior in altitude,

judging from their appearance from the mountains of Harris, from which they are seen rising in the west, like two isolated mountains from the bosom of the Atlantic. The shores are so precipitous, that, in Hirt, there are only two landing-places where a boat can be drawn up, the most convenient of which is situated in a bay on the south-east side of the island.

Snow very seldom lies to any depth, or for any great length of time, insomuch that, often when the mountains of Harris and Skye are covered, the St Kilda Isles are merely capped. Of rain, of course, they have an abundant supply, in common with the other islands. The surface is in general rocky and irregular: the soil is tolerable in some places, and there is abundance of peat, though it is not very deep. The pasturage is good, and sufficiently well adapted for cattle.

This remote and solitary group is the resort of numberless birds, which retire thither to breed. The greater part of these is migratory, and especially those which are objects of pursuit to the inhabitants; the Solan Goose, *Sula alba;* the Fulmar, *Procellaria glacialis;* the Puffin, *Mormon fratercula;* the Guillemot, *Uria Troile;* the Auk, *Alca torda*, are the principal species. Next to these may be mentioned the Kittiwake, *Larus tridactylus*, which, as well as the above, is migratory. Of the permanent birds are various species of the genus *Larus*, *Carbo*, *Anas*, and many of the birds common in the Outer Hebrides. Of the rare birds which frequent these islands, one is particularly worthy of notice, the Great Auk, *Alca impennis*, a live specimen of which was, three years ago, brought to Harris by Mr Maclellan of Scalpay.

The inhabitants, which are to the number of about thirty families, are originally from the islands of Skye, Uist, and Harris, and consequently of the mixed race of Celtic and Norwegian origin, with which those islands are peopled. They are, in general, of ordinary stature, of fair complexion, and well-conditioned, rather sluggish in their motions, and of indolent habits. The language spoken is the Gaelic; but they have a

* This individual afterwards made its escape. As the Great Auk is now become a very scarce bird, it may be mentioned, that a specimen came to the hands of Mr Adam, Factor of the Lewis, last year, but being unfortunately in bad condition, the skin was ultimately turned out of doors: it was caught in a fishing net.

peculiarity of accent by which they are readily distinguished from the other islanders. In regard to disposition, they are accused of duplicity, and are possessed of cunning in a higher degree than might be imagined from their circumstances. They are peaceable, however, among themselves, and the exercise of a vindictive temper, or a sudden burst of passion, has never been known to produce fatal effects among them, and scarcely even the more ordinary application of violence.

In respect to food and clothing, the natives of St Kilda are in a much better condition than the poor people of some of the nearest islands. They cultivate bear or bigg, oats, and potatoes, which thrive pretty well, especially the former. Their system of agriculture is the same as that which prevails over the Outer Hebrides in general, the *caschrom* or *crooked spade* being the principal implement employed for turning the soil. Black cattle and sheep they possess in considerable abundance, especially the latter. They have also a few horses, but these animals are of little comparative value to them. Their cattle, in general, are small, more particularly the sheep, the wool of which is of various colours, a light reddish-brown being very prevalent. Of food they also procure a very considerable supply at the expence of their visitors the sea-birds, robbing them not only of their eggs in vast quantities, but also of their lives. The eggs of sea-fowl, in general, it is well known, are excellent food, and the St Kildians find their flesh not less delectable, especially that of the young of the solan-goose and fulmar, which they salt for winter provision. The sea abounds in fish of various sorts, particularly cod and ling, of which, many years ago, some Dutch crews availed themselves; but, as the natives do not venture to sea, excepting on their visits to Boreray, and occasionally to the Hebrides, their supply of fish is confined to that procured from the rocks, by the rod or hand-line, and even this is considerable.

Their huts are constructed of stone and turf, and thatched, like the huts of the other Hebridians. The walls being thick, consisting of two facings of stone, with the interval filled up with earth, they form recesses in them, covered above with slabs, on which they sleep. The windows and chimneys are simple apertures in the roof. The fire is placed in the middle

of the hut. The cattle are arranged along the walls in winter, and their dung, mixed with straw, together with the whole filth of the dwelling, allowed to accumulate from one spring season to another, being only removed when about to be applied as manure.

The St Kildians are under the management of the tacksman, or head farmer, who rents the island of the proprietor, and who imposes upon them the conditions necessary for enabling him to fulfil his own engagements. Their rent is always and entirely paid in kind; feathers, grain, wool or coarse cloth, and oil, being the principal articles. The feathers are procured from the solan-geese and several of the smaller species: the havoc made among these birds for this purpose may be in some degree imagined. The oil is obtained from the fulmar, but is also partly prepared of the fat of other birds. It is of a clear amber colour, burns very brightly, with little smell, and is preserved in the gullets of the solan-geese, prepared for that purpose by infusion and drying. The solan-geese breed only in Boreray; and the natives are therefore obliged to make several visits to that island every season. It is needless to say, that they are all dexterous bird-catchers, and intrepid rock-men. For descending into the cliffs, they make use of a rope made of twisted thongs of cow-hide or of horse-hair, which they fasten above with a wooden peg. The solan-geese are always killed under night, and thrown from the cliffs next morning into the sea, where they are picked up by the boat. Accidents sometimes occur, and the unfortunate rock-man is precipitated from his giddy height, being generally killed before he reaches the sea, by dashing against the projecting edges of the cliffs.

The tacksman, who resides in some of the Hebrides, makes two voyages annually, one in June, the other in August or September. He carries to them the various articles that are most indispensable for their comfort, such as iron, spades, wood, sickles, handkerchiefs, hats, &c., and receives in return oil, mutton, cheese, butter, wool and coarse cloth. Of tobacco and spirits they are very fond, but their inclination is seldom much indulged in that way.

The St Kildians have hitherto had a clergyman resident among them, whose duty was also to act as schoolmaster. In

regard to religion, we can say little with certainty, and regret to understand, that, of late years, they have profited but little by their intercourse with their clergyman, with whom they would seem to have lived in a state of mutual hostility. They are civil to strangers; but do not seem to be attentive to their own poor, who generally leave the island, to wander over Harris and Uist. We can readily imagine how useful a respectable clergyman would be among these poor people. In the relation which might be made to exist between them, we can contemplate by anticipation, the pleasing picture of a sort of patriarchal government, in which the pastor, ever anxious to extend the illuminating influence of the Gospel over minds enveloped in darkness and superstition, might also foster a spirit of improvement in regard to their temporal concerns, which might materially improve their condition, while his people would repay his solicitude with affection and gratitude, more pleasing to a feeling heart than any recompence.

The St Kildians are fond of music and poetry. There are a few musicians among them, and the art of making verse, like many other trades in the Hebrides, can, upon any occasion, be taken up by every body. There is nothing, however, in the poetry to distinguish it from that of the other islanders. It is confined to songs, amorous and humorous, and lamentations for the dead, in which the feelings of the composer are first described, and then the good qualities of the deceased.

Two circumstances may still be mentioned with regard to St Kilda. The first is one, which, although firmly believed by the people of the neighbouring islands, and in fact attested by pretty good authority, is yet somewhat too marvellous to be readily credited. It is, that, on the arrival of strangers among them, a sort of catarrh is immediately diffused among the inhabitants. This catarrh they distinguish by a specific appellation, signifying *boat-cold*. The other circumstance relates to ornithology; it is, that, at an early hour, the solan geese which frequent the shores of the Outer Hebrides, are seen coming in strings from the north-west up the Sound of Harris, and in the evening, retiring toward the Atlantic in the same direction and order. If we consider the amazing velocity with which birds fly, it will not be considered an improbable conjecture, that these animals resort to Boreray every night, and return in the

morning to the fishing stations; in favour of which opinion, it may further be mentioned, that no instance has ever occurred of a solan-goose having been observed to repose on any of the shores of the Hebrides.

Such, then, is the condition of the inhabitants of St Kilda. Compared with that of the poor people of Harris or Lewis, it is in many respects enviable; how much more so, then, when compared with that of the miserable dwellers upon the frigid and desolate rock of the Iceland Grimsey. The St Kildian never quits his ocean-isle but with reluctance, and if detained by contrary winds, languishes and desponds: but this may be equally remarked of the natives of many of the other islands.

ART. XV.—*Report on the Present State of the Wooden Bridge at Montrose, and the practicability of Erecting a Suspended Bridge of Iron in its stead.* By GEORGE BUCHANAN, Esq. Civil Engineer, and Lecturer on Mechanics at the School of Arts, Edinburgh.

[The erection of Iron-Bridges of Suspension forms a new and very important branch of practical mechanics. On this account we have been careful to collect and publish in our previous numbers descriptions of the principal structures of this kind erected in this country. As the principles of their construction, however, have not been fully treated of in any work on this subject with which we are acquainted, we are happy to be able to lay before our readers a luminous and accurate exposition of these, in the following Abstract of a very interesting and able Report, drawn up by Mr BUCHANAN, at the request of the Montrose-Bridge Commissioners, and which they have printed for their information, and obligingly communicated at the author's request, along with the use of the Plates, for this Journal. The notes do not occur in the printed Report, but have been added by Mr Buchanan.—EDIT.]

THE river South Esk at Montrose, over which the present wooden bridge [1] is erected, is remarkable for its broad, deep, and

[1] This bridge was erected about the year 1792, prior to which period the river was crossed by means of ferry-boats. Owing to the rapidity of the currents, how-

very rapid stream. But the great width of the river, and the current, deep and rapid beyond example indeed in this country, are not owing to the magnitude of the South Esk river itself, but to the singular manner in which the discharge of its waters into the sea is here combined with the action of the tides, and the configuration of the adjacent ground.

The town stands on a gently rising ground, in one of those low sandy flats, which occur so frequently on the shores of the German Ocean, and which, from their slight elevation above the sea-level, and other circumstances, appear to have been once overflowed by the water. It has the German Ocean on the east, at the distance of about half a mile, and to the west is a tract of low and level sands, above four square miles in extent, and nine miles in circumference, through which the South Esk winds its way to the sea, passing close to the town on its south side. These sands lie below the level of high water and above the level of low water, and the river opening a communication with the sea, it necessarily happens, that every rising tide rushes up the channel of the river, and inundates the whole of this sandy flat to the west of the town, which is again left uncovered by the reflux of the tide. The channel through which this great body of water is alternately poured in and discharged, is suddenly contracted at the south end of the town, to the breadth of 700 feet at high water, and 400 feet at low spring-tides; and in consequence of this, the stream rushes in or out with great violence, according as the tide is either flowing or ebbing, and it is over this narrow part of the channel that the bridge is erected; the narrowness here, which both strengthens and deepens the current, rendering the situation in other respects favourable for a structure of this nature.

This low land, over which, at each return of the tide, are spread the waters of the ocean, after they have made their way through the narrow channel of the South Esk, is called the Basin, which forms a striking object in the scenery of the place, appearing, when the tide is full, a large and beautiful lake, and in a few hours afterwards, when the waters have retired, a de-

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ever, the passage of the ferry was often attended with danger; and as it lies on the great road between Edinburgh and Aberdeen, it was at all times felt as a great inconvenience.

solate and sandy marsh. (See Pl. VII.) But what we have chiefly to consider here, is the difficulty of finding in such a situation any solid foundation for the erection of a bridge. In consequence of the violence with which the water flows out and in, as the basin is alternately emptied and filled, the natural channel of the river has been deepened, and now contains in the middle parts a body of 30 or 35 feet of water at full tide, and never less than 20 feet at the lowest tides. The current also often runs at the rate of five or six miles an hour. The river Thames at London is broader than this stream, but its depth is much less, and its current is not nearly so rapid. Estimating the average depth of the basin to be six feet at high water, and its area four square miles, we shall find that the body of water discharged through this channel, is equal to that of a river which drains a country 10,000 square miles in superficial extent, a surface far exceeding that which is drained by the principal rivers of our island, and more than double the area drained by the Thames itself. It is not therefore merely a fresh-water stream over which we have to build, nor yet, strictly speaking, an arm of the sea. It is a vast river from the ocean, pouring in with rapid stream on our works, and in a channel already of uncommon depth, and at the same time so confined, and of so soft and yielding a bottom, that the least contraction of the water-way is sure to deepen it still farther, and thus endanger any work erected on so precarious a foundation, should the attempt even succeed of founding and building, in a situation so replete with difficulty and hazard.

At the place where the bridge is erected, the river is divided by a small island called the Inch into two streams, which again unite at a quarter of a mile below the bridge. It is over the northern and larger stream that the bridge is erected, the dimensions of which have been already stated generally, and will be found more minutely laid down in the annexed engraving, Pl. VI. The southern channel is much smaller than the northern, being scarcely half its size, and is crossed by a stone-bridge consisting of only one arch, but so narrow, that it has contracted the channel of the river, to at least one-fourth of its original breadth. The consequence of this is, that the greater part of the stream which formerly ran through this channel has been diverted into the north-

ern branch already so very large, and of which the channel has been at the same time greatly contracted from a similar cause, namely, the erection of the present bridge over it, as will be seen by a reference to the drawing. It is only the middle part of the bridge which is of wood, the two extremities being built of stone, and extended into the river as far as low-water-mark. This stone-work is 150 feet in length, and yet it contains but one arch, of 40 feet span, all the rest consisting either of piers or of solid wall, by which the channel of the river is contracted in a very sensible degree. The arch, also, which is scarcely two-thirds of a semicircle, instead of springing from the level of high-water, is filled at high spring-tides to within two or three feet of the key-stone. This stone-work, therefore, in both these bridges, although well executed, is injudicious in the design; as it is in every case a dangerous experiment to contract, or even to alter in any respect, the natural channel and long established run of a river, and more especially one of such magnitude, where the uncommon rapidity of the current, increasing in proportion as it is contracted within a narrower compass, is sure to tear up the soft and yielding bottom, on which it is continually working. The effect here has accordingly been exemplified in a manner well worthy of attention. It appears from the soundings of the river which I have taken, compared with the drawings that were made about the time the bridge was projected, that the channel on the northern side has been since deepened in many parts five and six feet. Immediately in front of the north pier also, as appears from the drawing, and was most sensibly felt with the sounding line, an unnatural steepness occurs in the bed of the river. In sounding, the depth was found suddenly to increase from 3 feet, at low water, to 12 and 16. The reason of this sudden rise in the ground is obvious. The violence of the stream having deepened the channel by carrying away the bottom, and gradually encroaching on the stone-pier, the foundation of which it threatened to undermine, it was judged expedient to drive in wooden piles for the protection of the bridge, by which the current having been suddenly repelled, and the ground preserved, the consequence is, that sudden inequality observed with the sounding line; the place where the wooden piles are driven in, presenting in some de-

gree the appearance of a wall in the bottom of the river. The pier, even with this protection, appears to have already subsided considerably, and, carrying the arch along with it, has torn the whole mass from the side-walls, forming a rent from top to bottom, and, at the footway of the bridge, several inches broad; and it is clear, that had the river been allowed to take its natural course, the whole of this pier would have been by this time undermined. The upper dotted line, which I have drawn on the section, shows the former course of the channel, and the under line represents what it would certainly have come to, but for the precautions taken to prevent it. It appears also from the information of Mr Smith, who is entrusted with the repairs of the bridge, and of others who have good opportunities of observation, that the channel has been regularly deepening for many years back. Many of the decayed piles which it has been found necessary to take up and to replace with sounder timber, have been found scarcely penetrating the soil, although they must have been originally driven at least to the depth of four or six feet.

It is remarkable that this deepening of the channel has taken place chiefly on the north side; and this appears to me principally owing to the contraction of the Inch channel by means of the bridge, the current of which being now directed chiefly into the northern channel, and winding round the Inch Island, both in running up and in running down, takes a direction inclining somewhat towards the north, and bearing directly on the northern pier; and although not reaching it by its own stream, yet throwing the great current of the basin in that direction, and thus wearing slowly and imperceptibly to the north, the established course of the river. The sea-worm is, as I am informed by Mr Smith, a kind of marine centipede which works only at the bottom of the sea or surface of the channel. Many of the piles which have been taken up, have been found accordingly sound enough, and nearly of the original dimensions, to within a few inches of the bottom, where the substance of the pillar has been so consumed, as in many cases not to exceed three or four inches in diameter, in a pillar, perhaps originally 12 inches square, or upwards, throughout.

In regard to the wooden part of the bridge, it appears also to be but ill-suited to the situation in which it is placed. After describing particularly the construction of this bridge, and remarking thereon, the author proceeds—

Such being the present condition of the bridge, the nature of the currents running under it, and of the bed of the river itself, it is become a matter of serious consideration to devise the most eligible plan for improving the passage of the river, and for reducing, if possible, the annual expence of repairs. If a bridge were proposed wholly of stone, it would require to be erected on a very different principle from that already adopted in the present structure. It would be necessary to spring all the arches from high-water-mark, and, according to the usual and approved mode of bridge-building, it would require altogether six or seven arches to span the whole width; seven piers, therefore, would be necessary, and these would contract the water-way about 80 feet. To guard against the deepening of the channel, which would take place in consequence of such contraction, these piers would require to be founded at least four or five feet below the bed of the river, not to be undermined by the increased velocity of the current, which would be sure to take place; to be laid also at this depth, on a broad basis of planking and piles, and all this to be done at an average depth of 20 or 30 feet under the surface of the water, and in a current of uncommon rapidity. These middle piers, again, would require to be raised 30 feet above the bed of the river, before any of the arches themselves could be sprung from them. The arches would require to rise at least 30 feet more, and thus the roadway of the bridge would be mounted in the centre to double the height of the top-beams of the present draw-bridge. All these, and many other circumstances, would be attended with such inconvenience, delay, and, above all, such an expence, of which, indeed, double or triple the amount of the present toll-dues would hardly defray the interest, that it is only necessary to bring these objections under the consideration of the Commissioners, in order to show the impracticability of the whole scheme.

A wooden bridge, therefore, of more approved construction, would be the only expedient, were it not that the material of iron, already applied with success to the purposes of bridge-

building, presents new resources in this, as in every other branch of practical mechanics, into which it has been introduced. A cast-iron arch, or arches, however, would still require intermediate piers similar to those already described, and the roadway to be elevated at least 20 feet above the present level, and would form altogether a work of greater magnitude and expence than what I have reason to think the Commissioners would be inclined to undertake. For this reason, and not having been required to do so, I have not entered into any particulars as to the most proper design of this kind, nor into any estimate of the expence; but have turned my attention chiefly to the only other construction which remains to be considered, and to which we are thus led, not from the novelty of the scheme, or even from choice, but rather from the peculiar circumstances of the case, to which this construction alone seems adapted, namely, the Pendent Bridge of malleable iron; that sort of hanging or inverted arch, where, instead of founding the piers or abutments as low as possible, and carrying the arch and the roadway which rests on it to a great height above them, two or more supporting pillars are raised on high, one at each end of the bridge; the arch, or iron-chain-work, is suspended from the top of these pillars, and hangs down between them, so that in the central part it is on a level with their base; and, lastly, the roadway, hanging from this main chain, and running everywhere on a level, is kept as low as the surface of the water will permit. On the fullest consideration, I am of opinion that this construction is the most advisable to be erected in lieu of the present bridge of wood: and I am the more induced to propose it, being convinced that the present state of the arts, and it may be added also of science, will enable us to raise a structure of this nature, superior in point of strength, stability, and accommodation, to any thing of the kind which has yet been erected.

The general construction of such bridges will be seen by the annexed drawing, Plate V.; but, as their nature is not generally understood, I may remark, that the suspended arch is, in most respects, the reverse of the common arch. The common arch, it is well known, is only sustained in consequence of the nice balance and adjustment of its several parts. But the materials of the arch and its road-way lying above the abutments from which

the arch is sprung, the whole rests or stands on these supports; and on this account the equilibrium of the arch is of an unstable and precarious nature, so that whenever it is disturbed beyond a certain point, it is sure to be completely overturned. When the bridge, therefore, is overloaded in any part, the fall of the whole structure is inevitable. The suspended arch is the reverse of this, and has, in this respect, the advantage. There is here also an equilibrium between the different parts of the structure, but as the materials of the arch and its roadway lie, in this case, below the suspending pillars, the whole rather hangs than rests on these pillars, and the equilibrium is, on this account, remarkable for its stability. It is that kind well known in mechanics under the name of the Stable Equilibrium; while the other, being less sure, is entitled the Tottering Equilibrium. The one cannot be overset, but the other requires considerable skill to be maintained. While the raised arch, therefore, falls irretrievably in consequence of any overload, the hanging arch cannot give way until the materials themselves which compose it are torn in pieces. The one tumbles long before the arch stones are strained to their utmost; while the other carries the load on its chains to the very moment of their fracture. In the one case the materials may be driven out of their place, and thus the structure may be overturned; while in the other, we have the whole strength of the materials as a security against accidents. And although the common bridge is, no doubt, seldom known to fail, its strength and solidity are chiefly owing to the enormous mass of materials which compose it; while the suspended bridge, on the other hand, is distinguished by the lightness of its structure, and the apparent boldness of its design.

But malleable iron possesses a peculiar advantage in its tenacity; it stretches long before it actually breaks; it begins to stretch, indeed, with about one-third, or, at most, one-half of the weight which would tear it asunder; and, as it is unsafe to load it with any more than this third or half of its utmost strength, at least if we wish to give permanence to the structure, this circumstance ensures the highest degree of security that can be desired: for even if the strength of the iron should happen to be estimated too low, which, however, cannot take place, if we are careful to prove each link of the chains before they are

put together; but should this improbability even occur, and should the load, for example, of an assembled multitude begin to stretch the chains, this circumstance could not fail to be observed. The sinking of the roadway would signify the approach of danger long before any accident could happen: so that if the structure be combined, with proper attention and skill, the actual breaking of the chains is a contingency which cannot take place by any possible concurrence of accidents.

In such bridges, then, it appears the utmost security is obtained in the arch itself, by proving carefully the strength of every bar or bolt of iron, of which the main chains are composed, and also of every joint or fastening, by which these detached bars or pieces are united into one great suspending chain, reaching from end to end of the bridge, there passing over its supporting pillars, and terminating in the ground, on each side. By thus knowing exactly how much each chain will bear, we obtain a safe rule for applying such a number as will cover every emergency.

In regard to the towers for supporting the bridge on each side, such strong and substantial pillars of stone and iron can be erected for this purpose, as to render it impossible that they should be crushed under the load they have to sustain. These can be founded also on so broad and ample a basis of pile-work as to prevent them from sinking: and, lastly, they may be secured at the bottom by such sufficient fastenings, that they cannot be overset by any external force or violence to which they are ever likely to be exposed.

To secure the extremities of the chains, these can be carried so deeply under ground, and there bound securely to a platform of pile-work, so firmly and deeply rooted into the soil, and so heavily loaded with stones or gravel, that the chains themselves would be torn in pieces before the load which stretches them could either disengage the fastenings, or pull up the piles. In all these cases, it is only necessary to know exactly the load which is ever likely to be laid on the bridge, or the nature of any external violence which may be apprehended, and to guard these vulnerable points of the structure, by a degree of strength in the several constructions, proportioned to the strain which each has to bear.

Next, however, to the perfect security of every part of the bridge, we must look to the accommodations; and in this respect the suspended bridge is every way on a par with the common bridge, excepting that the flexibility of the arch subjects it to a certain unsteadiness, which, in some of these structures, has been felt as an inconvenience. This, however, has chiefly arisen from want of the proper means having been taken to prevent it; and if such precautions be used, as I shall afterwards describe, I have no doubt this inconvenience will be completely removed.

In the erection of a suspended bridge, the first object is to ascertain the utmost strain to which the chains will ever be subjected. The strain arises from the weight of the roadway, and of any load of carriages or people that may be laid upon it, together with the weight of the rods by which it is suspended from the main chains, and also the weight of those chains themselves; each and all of these together being sustained by the cohesive strength of the iron, of which those principal chains are composed, and which is stretched in every part of it, exactly as if the chain were hung perpendicularly, and a weight, corresponding to the strain on the bridge, suspended at its lower extremity. It is of the first importance, therefore, to know the total amount of all these different weights, that we may be able to apply a chain, whose collective strength may be amply sufficient to sustain it permanently, without stretching or altering, by the strain, the natural texture of the metal. In the bridge now designed, the roadway is to be 30 feet wide, 28 feet clear of the chains and side-rails, and 20 feet clear of the two footpaths, each of which, therefore, is four feet wide. For the support of the roadway, there are to be only two sets of main chains, one at each side of the bridge, and running in a line above the outside of the footpath, and above the side-rails; so that between the opposite railings of the bridge all is clear foot and carriage way, the footpaths being only raised above the carriage way, and protected from the encroachment of carriages, by short iron-posts planted at convenient distances, and connected together, if necessary, by a chain, over which one may easily step.

I propose making the roadway wholly of iron, consisting of a series of iron-plates, supported on a frame-work below, and covered above with a coat of gravel or small metal, four inches

thick or more. The weight of the iron-work in the plates, frames, side-rails, suspending and cross rods, and, in short, all the iron-work supported by the chains together, I estimate at 180 tons, and the weight of four inches thickness of gravel at 220 tons.

The greatest load that can ever be laid on the bridge is when it is crowded with people, and I estimate the utmost number that it will hold at 7000, which is more than half the whole inhabitants of the town. Suppose each person 150 lb., or 12 stones and upwards, the total will be nearly 470 tons weight. The weight of the chains themselves will be 100 tons, so that, on the whole, in the most extreme case that can occur, we shall have loading and straining every inch of the chains,—viz.

Iron in roadway and in suspending rods,	180 tons.
Gravel, - - - - - -	220
People, - - - - - -	470
Chains, - - - - - -	100
Total,	970 tons.

We shall be enabled to lay this great load upon the chains with perfect safety, from the circumstance that, owing to the deepness of the curve, this load will produce no undue strain upon the arch; it will just stretch the chains as if they had been hung perpendicularly, and the weight suspended by their extremities. In the middle of the arch, indeed, the strain on the chains will scarcely be so much as this. This advantage is owing entirely to the height of the towers; for had these been so low as those of the Tweed Bridge, compared with its span, the weight of the roadway would have produced on the chains, owing to the flatness of the arch, a strain almost double that of their natural weight, and the carriages and people would all of them have produced an augmented strain in proportion; so that, instead of 1000 tons, we should have had a strain of nearly 2000 tons on every inch in the length of the chains from pillar to pillar, and from thence to the ground *.

* This circumstance of the strain on the chains varying with the depth or flatness of the arch, even though the natural weight hanging upon them be in either case the same, arises, it is well known, from the principle of oblique action.

When

Good English iron will bear for any length of time, without stretching or altering its texture in any respect, at least 8 tons

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When a rope or chain hangs perpendicular, and carries a weight at its extremity, it is evidently stretched by a force exactly equal to the weight itself, because the cohesive strength of the rope being always exerted lengthways, this when the rope hangs perpendicular, is directly opposed to the gravitating action of the weight, and these opposite forces producing an equilibrium, must necessarily be equal. But when the rope is made fast at its extremities to two opposite points of suspension, and the weight suspended between them, the case is quite different: Here the rope cannot hang vertically as before, but branches off from the point where the weight is attached towards each point of suspension. It thus divides itself into two distinct portions, each of which now bears its share of the load:—and the weight being thus sustained by two ropes instead of one, this, it may be imagined, should reduce the strain upon each one-half, and so it undoubtedly would, if each rope were to hang perpendicular. If the points of suspension were brought together, and the rope thus merely doubled, then it is evident that each half as it would only bear, so it would only be strained with the half of the weight. But when the points of suspension are removed from each other, and the rope spans the intermediate distance, then, as I have already remarked, the case is quite different; since the rope, instead of hanging vertically, must incline from the points of suspension towards the point where the weight is attached. But as the weight continues to draw directly downwards, and the rope to exert its strength directly lengthways, the forces are hence no more directly opposed to each other. Each branch of the rope bears obliquely upon its object, and before these indirect actions can become a match for the weight, which continues invariably to draw in the line of the perpendicular, they must necessarily acquire additional force, in proportion to their obliquity. If the rope, then, has just strength enough to bear the weight, when the points of suspension are brought together, it will be sure to give way the moment they are separated; and if it has sufficient strength to carry the weight, notwithstanding of this separation, each branch will yet be strained by a force, depending not merely on the weight itself, but also on the obliquity of the rope; the more oblique this is to the direction of gravity, the more will it be strained, according to the well known laws of oblique forces.

Such, then, is the reason of that remarkable strain which we observe in flat arches, whether suspended or raised, and of that moderate tension or compression which we obtain by increasing the depth or height of the curve. In the former, the arch hangs or stands every where extremely oblique to the direction of gravity; and the strain, therefore, on every part of it, must greatly exceed the natural weight which it supports. In deeper or higher curves, again, the arch hangs or stands less oblique to the perpendicular, the forces of cohesion or of compression, and the force of gravity, are hence more directly opposed to each other, and are therefore more nearly equal. In these cases, the weight is no doubt disposed over the whole length of the arch, instead of being accumulated in the centre; but the effect of this is merely to

of strain upon every square inch. Experiments differ in some degree as to this particular, some carrying the strength much

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alter the figure of the curve of equilibrium, in every part of which, however, the strain still invariably depends on the two circumstances already mentioned, namely, the weight which that part sustains, and its obliquity.

Without entering, however, into the mathematical laws which determine the direction of any part of the funicular curve, whatever be the disposition of the load, the strain on the arch of a suspended bridge may be easily determined by two simple, but important, considerations: 1st, In the middle or lowest part of the curve, the strain in different arches increases exactly as their depth is diminished, and diminishes exactly as their depth is increased, supposing the span to remain the same. In other words, this strain is inversely proportional to the depth, or versed sine of the curve: Secondly, When the depth is the eighth part of the span of the arch, then the strain in the centre is exactly equal to the whole weight of the bridge. In every arch, then, if the depth exceed the eighth part of the span, the strain in the centre will be proportionally less than the natural weight of the bridge; and if the depth be less than the eighth part of the span, this strain will be in the same proportion greater than the natural weight. So that, in general, if the depth be the nth part of the span, then in every case the strain in the centre will be equal to the whole weight of the chains and roadway, augmented or diminished in the ratio of 8 to n. Suppose, for example, the depth to be the 16th part of the span, then $n = 16$, and the ratio of 8 to n or 8 to 16, is the same as that of 1 to 2. In the centre, then, the arch will be strained with a force equal to double the natural weight of the bridge. Suppose, again, the depth equal to one-fourth of the span, then $n = 4$ and the ratio of 8 to n or 8 to 4, is the same as that of 2 to 1, or 1 to $\frac{1}{2}$, so that here the horizontal tension is only the half of the weight of the bridge. Let W denote the weight of the bridge and its load, and T the strain in the lowest part or the horizontal tension, then $T = \frac{n W}{8}$.

The horizontal tension, or the strain in the centre, is less than the strain on any other point, as this increases gradually towards the points of suspension, where it is greatest of all. To find the strain there, however, we have only to add to the horizontal tension the nth part of the weight of the bridge; so that, if t denote the strain at the point of suspension, $t = \frac{n W}{8} + \frac{W}{n}$.

The arch of the chains is usually considered as a catenary curve, and even in this view the above propositions are sufficiently near the truth for practical purposes; for, when the depth is the eighth of the span, the strain in the centre, calculated in this manner, is only about $\frac{1}{30}$th part too little. But they will be found much more exact, if we consider that, in suspended bridges, the curve is in general much nearer to a parabola than to a catenary, as the level roadway tends always to bring it nearer to this latter figure, in proportion as its weight exceeds that of the chains. The arch, in fact, is only a catenary, if we suppose the weight of the roadway to be as nothing, compared with that of the

higher; but I have thought proper to take the lowest estimate. Should I, however, be entrusted with the erection of the proposed

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chains; and it is an exact parabola again, when the weight of the chains is as nothing compared with that of the roadway; and the latter is in general by much the nearest to the true state of the case. In the bridge above proposed, the weight of the roadway itself is four times that of the chains, and, when loaded to its utmost, it is ten times greater. This consideration is of importance in practice, much embarrassment having been felt in loading the bridge with its roadway, by the weight gradually altering the figure of the curve; and this will always take place if the lengths of the suspending rods be drawn to the curve of the catenary. As the progress of loading proceeds, the figure of the arch will change, the roadway will be drawn off its level, the suspending rods off the perpendicular, and the whole structure will be distorted, besides that different parts of it will be strained beyond what they are intended to bear. To avoid such evils, the length of the suspending rods must be calculated by that figure to which the weight of the road-way will finally bring the chains; and, for this purpose, the curve of permanent equilibrium cannot be too nicely investigated. But for calculating the least and greatest strains in the arch, we may, without sensible error, assume it as a parabola. Now, in all these funicular curves, whether catenary, parabola, or any other figure, it is a general property that the horizontal tension is proportional to the radius of curvature at the vertex; and is, in every case, equal to the weight of as much of the curve as is equal to the radius of curvature in length, and of the same thickness or cross section as the curve at the vertex. But the radius of curvature of the parabola at the vertex, is just half the parameter; and hence, from the well known equation of this curve $y^2 = px$, we deduce T, the horizontal tension $T = \frac{1}{2}p = \frac{y^2}{2x}$. But y is half the span of the arch $= \frac{1}{2}S$, S denoting the span, and x is its depth or versed sine $= d$; hence $T = \frac{S^2}{8d}$. Let d now be $= \frac{S}{n}$, and $S = nd$, then $T = \frac{nd \times nd}{8d} = \frac{nS}{8}$. But S denotes the weight of a part of the bridge, equal to the span in length, and having every where the same cross section as the chains and roadway in the middle; and this is evidently within a mere trifle of the whole weight of the bridge, only falling short of it by the weight of a part of the chains, equal in length to the difference between the arch and the span, and which will not, in general, amount to the 200th or 400th part of the whole weight. For S, therefore, we may safely substitute W, and this gives the formula $\frac{nW}{8}$, already stated.

The same rule may easily be deduced by the principles of fluxions. It is evident that the fluxion of the ordinate, is to that of the abscissa as radius to the tangent of the angle which the curve makes with the ordinate, and which is termed the Angle of Deflection; that is, $dy : dx :: R : \text{Tang. } D$. But as the weight of each half of the bridge, according to what we have seen, is equal to the ordinate y; and as this weight is also in every funicular curve,

bridge, I would make, with an improved apparatus, such trials as would decide beyond all question, the strength of the iron to be employed in the construction of the bridge in question, more especially as this could be done at a trifling expence. In the mean time, we may safely assume the strength at 8 tons to the inch, which is the lowest estimate I can find, and which is only one-third of what the iron really will bear without breaking, as it takes from 20 to 30 tons to tear it asunder. Suppose, then, that the bridge is strained with the utmost load of carriages and people that can ever be laid on it, I propose having such a thickness of iron from pillar to pillar to support this load, that no part of it will ever be stretched with a force of more than 8 tons to the inch; and this it will endure, not only for the moment during which this extraordinary strain is ever likely to take place, but although it were continued for any length of time; so that any notion of danger from the utmost load to which it can ever be subjected seems totally out of the question; and much less can any hazard be incurred from the every-day traffic on the bridge. The thickness of iron required for this purpose will be about 124 inches, or about 62 on each side of the bridge. There will in fact be less strain by 100 tons in the centre of the arch than at each point of suspension, and the section of iron will require to be varied on this account, but 124 inches is the average; and could we stretch, therefore, one whole arched bar of iron, of this thickness, from pillar to pillar, and from thence to the ground, this would form the most perfect suspending arch. But this is impossible; such a vast mass can only be formed by uniting small pieces together, and it is of importance to consider what is the most convenient size for these pieces. In the Tweed

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equal to the horizontal tension into the tangent of the deflection, hence $y = \text{T tang D}$, and $\text{T} : y :: \text{R} : \text{Tang D}$. Thus, we have $dy : dx :: \text{T} : y$, and $y\,dy = \text{T}\,dx$, which, by integration, gives $\frac{y^2}{2} = \text{T}x$, and $\text{T} = \frac{y^2}{2x}$, as before.

For further information on the subject of the catenary, I may refer to Professor Leslie's Geometry of Curve Lines, and Elements of Natural Philosophy, where the strain in the lowest part of the catenary is expressed by a very simple, yet extremely accurate formula, viz. $\frac{b^2}{8d} + \frac{d}{6}$, b denoting the span, and d the depression.

bridge, the chains are formed by round bars or bolts, 15 feet long each, and 2 inches in diameter, each weighing about 2 cwt. The advantage of these large and heavy bars is, that they save joinings; but they are attended with other inconveniences, which more than counterbalance it. I should prefer bolts of smaller diameter, not exceeding 1$\frac{1}{8}$ inch; these can be more easily procured, are easier wrought, and easier put together, so that on the whole, they are attended with less expence. Another advantage arises from the smallness of these bars; their great number enables us to blend together in every part of the compound chain, the various qualities of the iron, so that if some bars should be below their proper strength others will be as much above it, and the collective strength of the whole will still be in every part of the arch rather above than below the standard. A greater number of joints will no doubt be required for the small bars; but this is a matter of little importance, more especially as these can be made of so simple a construction that the bars can be put together and taken to pieces again with the greatest facility. *

Another important consideration regards the quality of the iron to be employed. Iron, it is well known, varies greatly in this respect, and may be had of very different degrees of strength and toughness, according to the pains bestowed in rolling and hammering it. But the additional strength acquired by this elaborate process, is less in proportion than the increased expence incurred by it; and this refined iron, therefore, is only necessary in cases where the whole stress is laid, and the whole strength depends, on a single or double bolt, and where also it is desirable to combine as much strength as possible with little weight. But in the proposed bridge, the strain will be divided in every part of the arch from end to end among 70 bolts and upwards, and a weight of 30 or 40 tons additional will scarcely be felt, and can produce no sort of inconvenience, but is rather of advantage, as it increases the mass of our structure, and adds to its steadiness. I should prefer, therefore, common rolled iron of good quality, and such as can be had ready formed in bars or bolts of requi-

* The roundness of the bar possesses also an advantage in exposing less surface in proportion to its solidity to the action of the atmosphere, than any other form. This at least renders it, under the same circumstances, preferable to square or rectangular bars.

site size and length *. This will support, with perfect safety, 8 tons upon the inch; but at any rate the strength of every bar can be proved before it goes into the bridge, and rejected if it is too weak; or else, as already remarked, the strong and the weak can be so blended together, that the strength of the compound chain may be rather above than below the standard.

It will require, in all, 72 chains, each $1\frac{1}{8}$ inch diameter, to produce that strength which I should deem necessary: that is 36 on each side of the bridge. These I dispose in a compact square, having 6 chains in breadth and 6 in depth, and packed as close to each other as the joints will permit, so that they will not occupy more space than a square of 15 or 16 inches. Each chain is composed of a series of bolts, 15 feet long each, united together, end to end, by a joint which is of the same construction in them all. This joint is formed by upsetting each end of the long bolt, and hammering on it a shoulder $1\frac{1}{2}$ inches long, and projecting $\frac{1}{4}$ inch all round, placing the shoulders of each two adjacent bolts together, and then forming a cast-iron socket to receive these shoulders, and hold them fast together, as seen in Pl. V. Fig. 1. These sockets are cast in two halves to the exact mould of the shoulder and bolt, which they embrace when put together, and they are then bound by hoops. Cast-iron can be loaded with the utmost safety, with 4 tons, if not 7 tons on the inch, so that if the block, exclusive of the hollow socket, has twice the thickness, or twice the cross section of the bolts, in every part it will be fully as strong; and if it have five times the section, which will really be the case, the bolt itself will give way before the block. But to obviate every objection to cast-iron, which can be so readily formed into those hollow blocks, while the malleable iron presents great difficulties in this respect, each joint itself can be easily proved as well as the bolt. These joints, in the different rows of chains, I propose disposing at equal intervals in the length, so that when viewed sideways, they will have a curved appearance, as seen in the principal drawing. But any other arrangement may be adopted that may be found more convenient, as this is a matter of no very great importance.

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* This iron possesses also the important advantage of withstanding exposure much better than the refined sort, which, in proportion to its fineness, appears to corrode more rapidly by the action of air and moisture.

(To be concluded in next Number.)

ART. XVI.—*Remarks on a paper by Mr Haidinger on the Crystalline form of the Sulphato-tri-carbonate of Lead.* By H. T. BROOKE, F. R. S.

AN abstract of a paper by Mr Haidinger on the crystalline form of the Sulphato-tri-carbonate of Lead, was published at page 286. of the last Number of this Journal. The paper itself has been printed in the Transactions of the Royal Society of Edinburgh. It will be seen from Mr Haidinger's statement, and from the other pages of this Journal to which he has referred, that I had supposed the primary form of this mineral to be a *rhomboid;* that Dr Brewster had described some optical properties belonging to it, which he conceived to be incompatible with that form; that Mr Haidinger has imagined its primary form to be derived from *an oblique rhombic prism;* and that three such prisms are asserted by Mr Haidinger to be combined in the production of those figures, which have the appearance of rhomboids, as seen in his figure 6, Plate X. Vol. X. of this Journal.

Since the publication of Mr Haidinger's memoir, I have again particularly examined my own and several other specimens of this mineral; and from the examination and the measurements of many different varieties of its crystals, I am satisfied, that Mr Haidinger has entirely mistaken its crystalline form, *and that not one of his figures can ever occur among the crystals of Sulphato-tri-carbonate of Lead.* By what accidental circumstance Mr Haidinger had been deceived, it is difficult to say, without seeing the specimens from which his notion of form has been derived; but that he has been so deceived, may be easily ascertained, by any person who will merely take the trouble of looking at a few specimens of the mineral itself, and of measuring the mutual inclination of a few of its crystalline planes. It is not my intention to enter farther into the examination of Mr Haidinger's paper, than to shew that it must be incorrect, from the actual figure and measurements of the crystals which he has professed to be represented in his fig. 6. On comparing this figure with the corresponding one in the plate, published

in the Transactions of the Royal Society of Edinburgh, I observe, that the lateral planes which, in fig. 6. of this Journal, are denoted by *b*, are marked by the letter *e*, in Part I. of Vol. X. of the Transactions, and, according to Mr Haidinger's theory of the structure of these crystals, the planes in question are those marked *e*, and not *b*, of fig. 2. in this Journal. I shall therefore consider these planes as if they were marked *e*, although I shall refer to them as planes *b*, from their being so marked in fig. 6.

It is not easy to understand Mr Haidinger's own ideas of the manner in which this theoretical crystal, fig. 6. is formed, from the explanation he has given in the 12th page of his paper, in the Transactions of the Royal Society of Edinburgh; but I think I do not misrepresent his meaning by stating, that in this figure the "faces of crystallisation on the other (under) side, are parallel to those considered above," so that *c* on the *adjacent* upper plane shall measure the same as *c'* on the *adjacent* plane below.

This being so, it follows that *b* on *a* will measure the same as *b'* on the plane below adjacent to *c*, and the measurements of the crystals would thus be

a	on *e* - -	112°
a''	*c'* - -	112
c'	adjacent plane below, which may be called a^2	112
a	*b* - -	90 14'
a''	*b''* - -	90 14
b'	a^2 below - .	90 14
a	*a'* or *a''* - .	179 10
b	*b''* *a* re-entering angle of -	180 20
	and consequently measuring by the goniometer - .	179 40
	the left hand *b'* on *b* -	119 40
	the upper *b''* on the right hand *b'*	119 40

The lines on the upper and lower terminations are supposed to be edges at which the planes *a* intersect each other at angles of 179° 10'. This angle Mr Haidinger says it is "very easy to overlook." From what experience Mr Haidinger entertains this idea is not expressed, but it appears to me that such an angle could not well escape the notice of even a moderately close observer.

If the reader will take the trouble of examining any speci-

mens on which the crystals purported to be represented by fig. 6. occur, he will observe, that they do not exhibit the slightest trace of salient angles upon the terminal planes, or upon the cleavage planes parallel to these, or of re-entering ones upon the lateral planes *b*; but that both the *terminal* and *lateral* planes are *distinctly single*, and frequently very brilliant, particularly in those crystals which are the most symmetrically formed, *and where, on account of their symmetry, Mr Haidinger's imaginary lines ought to be the more apparent.* If, after having inspected these crystals, the reader will measure them by the reflective goniometer, he will find, when the planes are bright, that *a* on *b* will measure 90°, and the planes *b* on each other 120°, or within a *very few* minutes of these angles. I have found on one crystal *a* on *b* measure exactly 90°, *b* on *b* within a minute 120°. The constancy of these characters in the apparently rhombic crystals, might alone be sufficient to disprove Mr Haidinger's statements, but I may add that they equally disagree with every other crystal that I have seen, and with every angle I have measured. One of the crystals I have observed, contains planes corresponding in position with *e*, *P*, and *P'* of Mr Haidinger, whose inclinations on *a* he gives thus

a on e	112°	0'
P	111	42
P'	111	18

My crystal measures

a on *e*	111°	28'
P	111	30
P'	111	28

I have also measurements of planes corresponding to his

m on P	107°	49'
P'	107	50

results which are wholly inconsistent with Mr Haidinger's notion of its form.

It is some satisfaction to me to know, that both Mr Levy and Mr Phillips consider Mr Haidinger's figures incompatible with the specimens they have respectively examined. I have stated, that it is difficult to detect with certainty the source of Mr Haidinger's misconception, without seeing his crystals, but there are

frequently small crystals of *carbonate of lead*, accompanying the sulphato-tri-carbonate, which present some striking analogies with Mr Haidinger's figures

A twin-crystal of carbonate of lead now lies before me, of which the plane of junction corresponds very nearly in position with that of Mr Haidinger's fig. 1., and where it passes through the lateral planes, an *obtuse re-entering angle* is produced. If the reader will refer to Mr Phillips's measurements of *carbonate of lead* in his Mineralogy, p. 339, he will observe, that there are two planes *i* and *m*, fig. 3, truncating the edge of *h*, and that Mr Phillips gives for the measurement of these planes on *h*

h on *i*	151° 21′	
h *m*	121 26	

If we turn to Mr Haidinger's measurements, we shall find he gives

a on *l*	151° 10′
a *m*	132 15
a *n*	121 12

Although I have repeatedly found a plane corresponding to his *m*, it is remarkable that it has never been accompanied by *l* or *n*. Still, however, these may exist on Mr Haidinger's crystal, and their near agreement in measure with Mr Phillips's *i* and *m*, may be merely accidental. I shall only add, that Mr Haidinger's oblique planes *d* are by no means incompatible with the secondary form of a rhomboid, when it is recollected that one of two symmetrical planes will frequently appear singly on a crystal. The plane *d* might belong to the modification I have denoted by *f*, an example of which occurs in Haüy's fig. 139. of carbonate of lime, in the late edition of his Mineralogy.

If the primary form, therefore, of the mineral in question be not, as the figures of the crystals indicate, a rhomboid, some other substitution must be made than that which Mr Haidinger has proposed.

Art. XVII.—*On the Influence of the Hygrometric State of the Atmosphere upon the Minimum Temperature of the Night.* By Adam Anderson, M. A. F. R. S. E., Rector of the Academy of Perth.

THE connection between the minimum temperature of the night, and the contemporaneous state of the air, in regard to humidity, was first pointed out, I believe, in the Article Hygrometry, which I wrote several years ago for the Edinburgh Encyclopædia. In that dissertation I remarked, that, as the temperature of every place for the whole year ranges between two extreme points, corresponding to the alternations of summer and winter; so it exhibits, during the diurnal rotation of the Earth upon its axis, a like difference resulting from the vicissitudes of day and night. In the case of the daily change of temperature, there is some interval between the maximum and the minimum condition, which may be regarded as the temperature belonging to the season of the year; and though that point is not, at all times, equally distant from the extremes between which it oscillates, it seldom departs far from their mean. If this mean temperature were to rise and sink regularly, as the year advanced and declined, without being subject to daily fluctuation, the quantity of moisture existing in the atmosphere, at any given time, might be determined by the thermometer alone, with considerable precision; as it would generally be less than the quantity corresponding to the mean temperature, and seldom greater than that which belongs to the minimum temperature, the latter setting limits to the accumulation of watery vapour in the atmosphere, while the former no less effectually secures it against a state of long continued dryness. The truth of these assertions will be readily perceived, by a comparison of the minimum temperature with the point of deposition, or the temperature at which the moisture existing in the atmosphere would begin to deposite itself. I shall select for this purpose the observations which my friend the Rev. Dr Gordon made in the year 1815, during his residence at Kinfauns; not only because they were made without any view to the support of a

theory, but still more because his extreme accuracy as an observer, joined to his profound knowledge of this department of physical science, gives them an authority which can be claimed for the materials of few meteorological journals.

	Mean Temperature of the Air.	Degrees of Leslie's Hygrometer.	Grains of Moisture in 100 Cubic Inches of Air.	Point of Deposition by Formula.	Mean Minimum Temperature.	Difference of Point of Deposition, and Minimum Temp.
January,	33°	4°.9	.1238	29°.1	28°.5	+ 0°.6
February,	42	8.1	.1612	36.8	35.7	+ 1.1
March,	43	14.7	.1451	33.8	34.9	+ 1.1
April,	47	23.1	.1458	33.9	36.9	+ 3.0
May,	54	23	.2050	44.3	45.5	— 1.2
June,	59	29.7	.2304	47.9	48.9	— 1.0
July,	61	32	.2444	49.7	50.3	— 0.6
August,	60	27.7	.2463	49.9	50.9	— 1.0
September,	55	20.3	.2214	46.7	46.9	— .2
October,	49	12.2	.1967	42.9	42.6	+ .3
November,	38	7.8	.1396	32.6	31.4	+ 1.2
December,	33	6.5	.1177	27.9	27.8	+ 0.1
	48		.1814	39.6	40.0	

The absolute humidity of the atmosphere, exhibited in the column entitled Grains of Moisture in 100 cubic inches of air, was computed by a formula which I have given in the article Hygrometry already alluded to, and the correctness of which I have since verified by a careful comparison of the results it affords, with the quantity of moisture in the air, as determined by direct experiment. The column shewing the mean point of deposition for each month, exhibits the temperature at which the air, at the time of observation, if cooled down to that point, would begin to deposite the vapour in admixture with it. The adjoining column contains the mean minimum temperature as given by a self-registering thermometer; and the last column shews the difference between the lowest temperature and the point of deposition. The coincidence is sufficiently great to establish the important fact, that the quantity of moisture dissolved in the atmosphere is so intimately connected with its minimum temperature, that the former may be readily inferred from the latter. It was not difficult, indeed, to arrive at the conclusion, that, as the quantity of moisture which can exist in

the atmosphere, under the vaporous form, is entirely regulated by temperature, and totally independent of chemical solution, the hygrometric condition of the air must, at all seasons, nearly correspond with complete saturation at the minimum temperature; more especially from the summer to the winter solstice, a period of the year when the accumulated moisture is returning back, by the decline of temperature, from the vaporous to the liquid state. In the present paper, however, it is not so much my design to illustrate this point, as to endeavour to shew that the hygrometric condition of the atmosphere exerts a considerable reaction upon the temperature by which it is induced; the humidity of the air producing, in this respect, upon its temperature, an effect analogous to that of the fly-wheel of the machine, which regulates and controls the operation of the moving power by which it is itself kept in motion.

Every person who is at all acquainted with the general doctrines of caloric, is aware of the fact, that liquids, during their transition to the state of vapour, always absorb a large proportion of the matter or substance of heat. This heat is not discoverable, in its combined state, by any of the ordinary means which we employ to detect the presence and intensity of that subtile agent. In the case of the vapour of water, it exists in union with the aqueous particles, in what is called a latent or concealed state; but the characteristic energy of its nature is only suspended or neutralized, not destroyed. The vapour, with which it is united, cannot recover the liquid form, until the watery particles have been disengaged from the igneous fluid, which being thus separated, resumes its wonted activity, and produces upon contiguous bodies, with which it enters into new combinations, the usual indications of its presence and operation. Thus, when a quantity of common air, completely saturated with humidity, at any particular temperature, is made to suffer sudden condensation, the extrication of heat from the compressed vapour is not only perceptible by the senses, but sufficiently great to set fire to inflammable substances, placed within the sphere of its activity. The effect may be ascribed in part, no doubt, to the compression of the air, and the change of capacity for caloric, which it undergoes by condensation; but it seems to be chiefly owing to the transition of the vapour to

the state of water, in consequence of the reduction of volume it sustains, the effect being much more limited when dry air is subjected to the experiment. The conversion of vapour to the liquid condition being thus necessarily attended with the evolution of heat; even when the experiment is performed upon a small scale, where a large portion of the effect is expended upon the sides of the vessel containing the vaporised air, it is obvious, that the great aërial magazine which encompasses our globe, cannot yield up the moisture with which it is, at all times, more or less copiously charged, without having its temperature extensively affected by the change *. Hence, if, by any great physical cause, the temperature of the air should be exposed to a sudden reduction, the extent of the effect would not only be modified, but in a great measure counteracted by the extrication of heat which would thus take place. In conformity with these views, it may be inferred, *that the greatest cold during the night should always be observed when the atmosphere is in its driest state; and, conversely, that when the air is extremely moist, there should be little difference between the temperature of the day and the night.* This conclusion, which will go far to explain the cause of the various deflections in the isothermal lines of Humboldt, I have verified by a vast multiplicity of observations, both near the level of the sea, and in situations 2000 feet above it; during the extreme heat of summer, as well as the

* The heat extricated by the condensation of vapour is beautifully exhibited by the following simple experiment:—Let a little moistened linen or paper be placed under a receiver, upon the plate of an air-pump, in which there is at the same time suspended a delicate thermometer; on exhausting the air, the receiver will soon be filled with invisible vapour, which will be more or less dense, according to the temperature of the apartment at the time of the experiment. In the course of two minutes, if the air-pump be a good one, the density of the vapour will reach its maximum state, for the temperature; that is, the space occupied by the vapour will be incapable of holding more moisture in solution,—consequently, when the stop-cock is turned, the air, which is allowed to enter the receiver from the outside, carrying along with it the portion of water which it retains at the time in the vaporous state, the whole of that vapour must suffer condensation. The sides of the receiver are accordingly suddenly bedimmed with moisture, and the thermometer at the same instant rises 5 or 6 degrees. This experiment affords a fine illustration of the well-known fact, that the formation of a cloud in the zenith of a clear and serene sky, never fails to raise the temperature of the air at the surface of the earth 2 or 3 degrees.

most intense cold of winter, when the air was loaded with vapour, not less than when it was almost entirely bereft of humidity; insomuch, that, by ascertaining the hygrometric state of the atmosphere in the evening, I have no difficulty in determining the minimum temperature of the ensuing night, the deviation seldom being beyond the limits within which the nicest thermometrical observations are made.

The agreement, however, which I have endeavoured to trace between the minimum temperature and the point of deposition, is, for obvious reasons, most remarkable from the end of July to the end of December, the temperature of the year being then on the decline, and rendering the relative humidity greater than during the other half of the year; but at no season is the deviation so great as not to indicate a mutual connection between them. It may also be proper to remark, that, after the temperature of the night reaches the point of deposition, which it seldom fails to do, the check to its farther diminution operates most effectually at the higher temperatures; because the quantity of vapour which passes into the liquid state being greater, it exerts a corresponding influence over the thermal state of the air, and thus prevents the lowest temperature of the night from ever being greatly depressed below the point of deposition. In the month of July, for example, when the mean point of deposition may be taken in this latitude at 45°, the quantity of moisture in the air is 0·2099 grains in 100 cubic inches; whereas, in the month of December, when the mean point of deposition is 15° lower, the quantity of moisture in the same volume of air is only 0·1278 grains. So that a depression of 1° of temperature in the former case, would cause nearly double the quantity of vapour to pass to the liquid state, and evolve a proportional quantity of caloric, to counteract any tendency (whether from radiation or any other cause,) which the air might have to a farther reduction of temperature. I shall not occupy the pages of this Journal with a tedious comparison of numerical results in support of the general positions which I have laid down, but confine myself to a few interesting details of some observations which I made, under circumstances peculiarly well fitted to subject the theory to a severe trial. These I shall briefly describe:

Having occasion, during the month of August 1822, to fix my residence for some time at Amulree *, a situation in the Highlands of Perthshire remarkable for the dryness and salubrity of its air, I carried along with me a number of good meteorological instruments, for the purpose of making such observations on the hygrometric state of the air, in that elevated part of the country, as might tend either to confirm or overturn my hypothesis. The state of the weather, which proved extremely variable, was highly favourable to the object I had in view, the thermometer having ranged, in that short interval, from 63° to 26°, and exhibited greater fluctuations than I have ever observed in the month of August. On the 13th the day was hazy, and the coincidence, the following night, between the minimum temperature and the point of deposition, as indicated by my formula, was exact. The following day the humidity of the air increased considerably, and the point of deposition rose from $45\frac{1}{2}$° to $48\frac{1}{2}$°. The ensuing night the minimum temperature was proportionally raised, the register-thermometer having indicated 50°. The next day, the absolute humidity of the air was reduced, in consequence of an impetuous dry wind, from .282 to .165 grains of moisture in 100 cubic inches of air, and the point of deposition thus descended to $37\frac{3}{4}$°. The minimum temperature which followed this very sudden change was $38\frac{1}{2}$°, differing only $\frac{3}{4}$ths of a degree from the temperature assigned by the formula. From that day the dryness of the atmosphere gradually increased till the 22d, when the quantity of moisture in the air became less than is commonly observed, except in the most rigorous of the winter months. On the evening of that day my formula indicated that the point of deposition was reduced to 26°, and the next morning I had the satisfaction to find that the register-thermometer had, in the course of the night, reached the very same point. Another register-thermometer, which I had placed on the summit of a neighbouring hill, at the height of 905 feet above the point where I usually made my observations, shewed that the greatest cold in that elevated situation had, contrary to what commonly happens, been 1° higher.

* Amulree is, by my barometrical observations, 935 feet above the level of the sea.

This remarkable degree of cold, at that season of the year, seems to have been in a great measure local, and to have extended only a few miles around Amulree. At 5 o'clock in the morning, the ground in the neighbourhood of that place was so hardened by the frost, that it could not be penetrated by an instrument; and the crops, particularly those of oats and potatoes, suffered severely; the former, which were previously in a greenish state, soon after assuming a pale yellowish appearance, while the latter were completely blackened and withered in the stem. The intensity of the frost was less felt in the neighbourhood of Crieff, which is about 10 miles from Amulree, but even there it sensibly affected the standing crops. Around Perth * its influence was scarcely perceptible, the register-thermometer having descended only to 39½°; but in the vicinity of that place the air possessed a much greater degree of humidity, the point of deposition, by the application of my formula to Dr Gordon's observations at the Manse of Kinfauns, being 38°.

The extreme dryness of the air, which preceded the great depression of temperature, during the night, around Amulree, was not of long duration. On the evening of the 23d, the point of deposition rose to 32°, and the minimum temperature of the following night did not descend below 30½°. Next day, the quantity of moisture in the atmosphere was more than doubled, so that the point of deposition, assigned by the formula, rose from 32° to 47°. The elevation of the minimum temperature corresponded to this increase of the humidity of the atmosphere, and rose from 30½° to 46½°. These facts seem to establish, in the most satisfactory manner, that a coincidence exists between the lowest temperature of the night and the point of deposition, or that temperature at which the moisture dissolved in the atmosphere would return to the liquid state. The only question with respect to which any difference of opinion can be entertained, is, Whether the temperature regulates the humidity, or the humidity the temperature? To this question I have no hesitation in replying, that, from the facts I have adduced, we are warranted in drawing the conclusion, that the temperature, in the first instance, determines the mean hygrometric condition of the atmo-

* Perth is 18 miles from Amulree, the first range of the Grampians intervening.

sphere, at least where the surface of the earth affords a sufficient supply of moisture; and that the latter, in its turn, re-acts upon the temperature, and prevents it from being in any case greatly depressed below the point of deposition. In cases where this check operates in a feeble manner, which must always happen when the atmosphere is reduced to great dryness by the translation of air from a higher latitude, or across elevated table-land, the lowest temperature of the night must always be considerable. In the months of April and May, for example, the air over our island is generally extremely dry, on account of the cold and parching north-east winds which prevail at that season, and the minimum temperature of the night is accordingly often reduced below the freezing point. Last season afforded a striking illustration of this. The spring was throughout extremely dry, and the minimum temperature descended almost regularly every night below the freezing point, the only exceptions having occurred when a partial change took place in the humidity of the air. Even on the day of the summer-solstice, though the maximum temperature rose at Perth to 70°, yet the moisture in the atmosphere being so scanty that the point of deposition was so low as 28°, the temperature of the ensuing night sunk to 30°; nor did the self-registering thermometer cease to approach the freezing point, until the absolute humidity of the atmosphere became permanently and considerably augmented.

From the remarks which I have made on this important subject, a method of determining the average hygrometric state of the air, by means of the minimum temperature, readily suggests itself, it being evident, that places, where there is little difference between the greatest heat of the day and the extreme cold of the night, must be exposed to a moist atmosphere, while those situations which possess a wider range, with respect to the diurnal returns of temperature, must enjoy a corresponding degree of dryness, and probably, too, of salubrity. But it is to the influence of moisture, as the means of checking the diminution of temperature during the absence of the sun, the great source of light and heat to our globe, that I wish chiefly to draw the attention of meteorologists. After the sun has sunk below the horizon, the temperature of the air, which had been previously declining with his altitude, undergoes a more rapid

diminution, and reaches its minimum state generally about midnight; after which it suffers little or no change till towards the hour of sunrise. From midnight the progress of the cold is almost completely arrested, the transition of the vapour to the liquid state evolving a sufficient portion of heat to counteract the loss of temperature by radiation, and the other physical causes by which the cold of the night-air is induced. Before sunset, a considerable portion of moisture, it must be admitted, is discernible on the blades of grass, and other spicular substances on the ground; but the dew thus formed is rather a condensation of the moisture exhaling from the surface of the earth, by the heat it had acquired during the day, than a precipitation of vapour from the atmosphere, which it would seem does not begin to take place until the temperature of the air has reached the point corresponding to complete dampness. This is the reason why from midnight to the time when the heat of the sun begins to be sensibly felt next morning, the temperature of the night is nearly stationary; and were it not for the constitution of things which I have described, the reduction of temperature would proceed to a much greater extent than is actually observed, seeing that the night-cold, even in the months of summer, when the season happens to be very dry, is always excessive, and frequently descends below the point of congelation, at the very time when the temperature of the day rises to 75°.

ART. XVIII.—*Notice of an Inquiry into the Principles according to which Friendly Societies* (or Benefit Societies) *ought to be conducted, in order to insure their stability and success.*

A VERY interesting publication, upon this important subject, is about to make its appearance, in the form of a Report from a Committee of the Highland Society of Scotland. The inquiry was moved for so early as 1819, in consequence of the well known facts, that, of the numerous Friendly Societies instituted at different times in this country, the great bulk had gone to ruin, by making greater allowances than their funds could afford; that of those which remained in existence, the major part had been compelled to lessen their allowances to such sums as their funds could bear; and that very few, indeed, were in a

state of solvency. Comparatively little blame, however, could be attached to the managers of any of them, as they were without any data from which to make calculations; but it seemed evident, that their experience, if it could be collected, might afford good data for the guidance of similar societies in future. With this view, the Highland Society appointed a Committee, Mr Oliphant, W. S., as Convener; and, in 1820, schedules were prepared and circulated in most parts of Scotland, accompanied with an exemplification, to enable members of societies to fill them up with accuracy, and with the least possible trouble to themselves. The Highland Society also offered premiums for the most distinct and satisfactory returns. By the end of 1822 returns were received from upwards of 70 societies, comprising the experience of about 7000 persons, which, when multiplied by the number of years during which they respectively were members, gives more than 100,000 results. The digesting and arranging of these, as well as the subsequent calculation of a great number of Tables founded upon them, was undertaken by Mr John Lyon, Governor of Watson's Hospital, a gentleman well qualified for such a task ; and in the execution of which, he had the advice and assistance of a number of gentlemen of great experience, as calculators.

The average annual Sickness, to an individual, is thus given in the Report.

Age.	Sickness in weeks and Decimals.
Under 20	0.3797 *
20 to 30	0.5916
30 to 40	0.6865
40 to 50	1.0274
50 to 60	1.8806
60 to 70	5.6337
above 70	16.5417 *

The total average sickness, experienced by a person who attains to the age of 70, during the 50 years from 20 to 70, is 98.197, or 98⅕ weeks. But the quantum of sickness alone does not afford sufficient data for the formation of proper tables. Mortality must also be taken into the calculation ; and consider-

* These being for indefinite terms of years, are not entitled to the same confidence as the intermediate Sickness, which is for uniform terms of 10 years.

ing that the Northampton Table, though it has long been used in calculations of this kind, gives the mortality by much too high, the Report proceeds upon an average of three Tables, the Northampton, the Carlisle and the Swedish. Interest is calculated at 4 per cent.

The Tables are constructed for four distinct Schemes, viz. 1*st*, Sickness from 21 to 70 ; 2*d*, Superannuated Allowance, or Life Annuity, above 70 ; 3*d*, Funeral Money, or a sum payable upon the death of a member ; and, 4*th*, Annuity to Widows of the first marriage only. The annual contribution to each scheme is L. 1 from each member, from the date of entry to the age of 70, if he lives so long. The standard age of entry is held to be 21 ; and every entrant, above that age, must either pay down at entry a sum equal to the individual stock of the members of his age, who entered at 21, or an increased annual contribution in lieu of it. Each of these is distinctly set down, in a separate column. It is found that this contribution, to each of these schemes, will afford, 1*st*, A weekly Sick Allowance, from 21 to 70, of L. 1 : 0 : 7 ; 2*d*, A Superannuated Allowance, or Life Annuity, to such of the members as survive 70, of L. 58, 0s. 2½d ; 3*d*, A Funeral Allowance, or sum payable at death, of L. 59 : 19 : 2 ; and, 4*th*, A Life Annuity to widows of the first marriage only, of L. 5 : 12 : 6½.

A number of interesting details are given in the Report, accompanied by rules and problems for converting these contributions and allowances into others, so as to provide for a very considerable variety of cases. Besides forming a prominent part of the sixth volume of the Highland Society's Transactions, we understand that, for the convenience of the numerous Friendly Societies in the country, the Report is to be published as a separate pamphlet, by Messrs Constable and Company.

ART. XIX.—*On the comparative Value of Oil and Coal Gas.* By ANDREW FYFE, M. D. F. R. S. E. Lecturer on Chemistry, Edinburgh.

AT a time, when the public interest is so much excited, no apology, I conceive, is necessary, for laying before them some

remarks on Gas-light; not with the expectation of advancing any thing new, but merely with the view of collecting into a condensed form, what has already been said on this subject, particularly on the important question, Whether oil-gas can compete with coal-gas, in point of economy.

Although, at the first erection of gas-works, the public feeling was strongly against them, on account of their supposed danger and offensive nature, this prejudice has gradually worn off. Though the danger of having large collections of gas has been rated at an enormous extent by Sir William Congreve, only one instance of the explosion of a gas-holder has happened; and this occurred in the first filling of one at Manchester, through ignorance, and the carelessness of the workmen. The atmospheric air not having been extracted, was, of course, allowed to mix with the coal-gas, and one of the men wishing to ascertain if the gas-holder was tight, applied a candle to a part, from which gas, *mixed with atmospheric air*, was issuing, which caused it to explode, and tear the gas-holder in pieces.

A few accidents have occurred, from the escape of gas from the pipes, but these have, in general, been produced by the carelessness of the workmen, and were of a trifling nature; for when the gas does escape, it is only when it gets into some confined place, as a vault, or a common sewer, that it can, on the approach of flame, do any mischief. It has been advanced by some, that accidents may happen from the gas escaping from the burners, by forgetting to turn the stopcocks. No accident of this kind is, we believe, on record; nor is it at all likely to happen. Shops and apartments of a dwelling-house are not close enough to keep the gas confined; but allowing that they are, the quantity emitted, is too trifling, compared to that of the air. Coal-gas is most explosive when mixed with about 5 of air. In a room, then, of twelve feet each way, one burner, consuming five cubic feet per hour, would be sufficient to light it, but, in this apartment, there are 1728 feet, so that, to get an explosive mixture, and allowing there is no loss of gas, the burner must be left open upwards of fifty hours, or at least two days and nights, which is not likely to happen. When more lights are used, the apartments are of course larger, so that the same time would still be required. In those cases, also, in which the burners are left open, the odour

of the gas gives warning of its escape; so that one of its properties considered offensive actually proves a valuable safeguard.

Of the Nature of the Gaseous Fluid emitted by the decomposition of Oil and Coal.

The gaseous matter, given off from coal and oil, now known by the name of coal and oil gas, contains nearly the same ingredients, but in different proportions. Dr Henry has shown, that they are mixtures of hydrogen, carbonic oxide, carburetted hydrogen and olefiant gas, with occasionally a little nitrogen; and in addition to these, coal-gas, before it is subjected to the process of purification, always contains ammonia, carbonic acid and sulphuretted hydrogen, but from which it is, or at least ought to be, freed before it is sent into the gas-holder, so that both gases, when exposed for sale, contain the same ingredients, but in different proportions. There is also given off, during the decomposition of coal and oil, an essential oil, which seems to be held in solution, in a state of vapour, in the gas, and which is the cause of the smell, and, as some suppose, adds to the illuminating power.

Dr Henry, in his paper in the Annals of Philosophy for September 1821, has given the component parts of different samples of gas. The coal-gas was prepared from Wigan Canal, at the manufactory of Messrs Phillips and Lee, and collected from an opening in a pipe between the retort and the tar-pit, generally about half an hour after the commencement of the distillation, except in the instance of the gas No. 4., which was taken five hours, and No. 5., ten hours from that period; the carbonic acid and sulphuretted hydrogen being removed by washing it with solution of potassa.

	Spec. Grav.	Gas Condensible by Chlorine.
Gas, No. 1.	650	13 per cent.
2.	620	12
3.	630	12
4.	500	7
5.	345	0

After the condensible gas was removed, there remained,

	Azote.	Carb. Hyd.	Carb. Ox.	Hydrog.	Total.
No. 1.	1.5	94.5	4	0	100
2.	6	82	2	10	100
3.	2	66	14	18	100
4.	5	60	12	23	100
5.	10	20	10	60	100

In the following Table, the oil gas of the three first experiments was procured from whale-oil, previously boiled, to free it from water, the heat of the retort being in each succeeding experiment reduced till it was just sufficient to decompose it. The fourth gas was from a London work.

	Spec. Grav.	Lost by Chlorine.
Gas, No. 1.	464	6 per cent.
2.	590	19
3.	758	22.5
4.	906	38.

After this there remained a gas composed of,

	Azote.	Carb. Hyd.	Carb. Ox.	Hydrog.	Total.
No. 1.	7	30	15	48	100
2.	5	40	15	40	100
3.	5	65	20	10	100
4.	5	75	15	5	100

The gas condensed by chlorine is supposed to be partly olefiant, partly a volatile oil. That not condensed, the above Tables shew to vary in its composition. In the best specimen of oil-gas, the carbonic oxide is in larger proportion than in the best kinds of coal-gas, and the carburetted hydrogen is most abundant in the latter. The hydrogen in both appears to increase as the temperature at which they are formed becomes higher, and is always greatest in the last portions.

On the Quantity of Gas obtained from Coal.

The quantity of gas to be got from coal varies according to the coal employed, and the manner in which it is treated; the quality also depends on the mode of applying the heat. Taking it for granted that the most advantageous method of decomposing it is followed, the quantity from the different kinds of coal varies. In stating the proportions, therefore, we can come only at an average conclusion.

Mr Peckston, in his work on Coal-gas, states, that a chaldron of Newcastle Walsend coal will yield 10,000 feet, supposing it decomposed under the most advantageous circumstances; 2 cwt. will, therefore, yield about 750 feet.

At Edinburgh, 2 cwt. of parrot-coal yield on an average 860 feet of gas *.

According to Mr Neilson, engineer, Glasgow, 2 cwt. of Lesmahago coal will produce 1080 cubic feet of gas, allowing $4\frac{1}{2}$ to each pound. Mr Russell of London has stated the quantity from Newcastle coal to be the same, $4\frac{1}{2}$ feet per pound. Mr Dewy, in a late paper in the Annals of Philosophy, asserts, that, at Liverpool, Mr King considers it good economy to procure 7000 feet from a ton of Wigan Orral-coal, making it only 700 feet from 2 cwt., a very little more than 3 feet per pound. He has stated also, that, at Glasgow, 1200 feet are procured from 2 cwt. of cannel-coal, which is considerably above that mentioned by Mr Neilson. From these various statements, the general conclusion has been drawn, that 2 cwt. of good coal ought to yield about 1000 feet of gas.

With respect to the quantity to be got from oil, this must of course also depend on the nature of the oil, and the manner of decomposing it. Mr Ricardo mentions, that, from repeated trials, in various oil-gas establishments, it has been ascertained that 1 gallon produces 100 cubic feet. From the experiments of Mr Brande and Mr Faraday, it appears that the same quantity affords from 100 to 110 feet. In some instances I have known it amount to about 120, but in these cases it was not good, the additional quantity having been derived from substances put

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* The gas yielded by this coal is found to be of superior quality to that from the other kinds submitted to trial.

into the retort At Leith, a gallon of whale-oil affords from 98 to 108 cubic feet; and the same quantity of palm-oil, from 97 to 114. It may be considered a fair estimate to obtain 100 feet from each gallon, presuming, of course, that the oil is decomposed under the most favourable circumstances, so as to get a gas possessing the greatest illuminating power, for on this every thing depends. From experiments I have performed on a small scale, and from trials made at Leith, I find that if the oil be allowed to flow into a retort brought just to a red heat, there is comparatively little gas, but a great deal of volatile oil. When the retort is brought to an intense heat, lamp-black is formed in considerable quantity; so that in both of these ways there is a great loss. When the retort is at a full red heat, the oil seems to undergo decomposition most easily, and to give off the largest proportion of good gas.

Illuminating Power of Oil and of Coal Gas.

Various statements have been given of the illuminating power of oil and coal gas; nor is this to be wondered at, when we consider that the quality of the gases depends so much on the mode of preparation, and take into account also the defective modes usually adopted for determining the intensity of the light afforded by their combustion. Mr Ricardo, in his early papers on this subject, has given a very flattering account of oil-gas. He states, that an Argand burner, giving a light equal to six candles, six to a pound, consumed one cubic foot per hour: and as Mr Accum mentions, that an Argand of coal-gas, giving a light equal to three candles, eight to the pound, consumes two feet in the same time, he has inferred, supposing the candles of the same size, the illuminating power as 4 to 1; but taking the average of a number of experiments, he has fixed their comparative power of giving light as $3\frac{1}{2}$ to 1.

In these trials, however, the gases were not brought into comparison with each other by burning them together, and the data on which he proceeds seem to be very fallacious, as it is not stated whether the candles were of the same kind in both experiments. Messrs Taylor and Martineau, the patentees of the oil-gas apparatus, have, however, come to nearly the same conclusion, that the illuminating power is as $3\frac{1}{2}$ to 1, a conclusion drawn from the experiments of Mr Brande and Mr Faraday.

A gentleman connected with the Liverpool Gas Company, in the answers to the queries put to him by the Committee of the Dundee Company, replies, that the relative quantity of gas requisite to supply the same light, is as 14 oil-gas to 51 coal-gas, making their power of affording light rather more than $3\frac{1}{2}$ to 1. Though the above statements place the illuminating power of oil-gas so high, a very different account is given by others. According to Mr Neilson, Glasgow, it is not to be rated at above 2, or at all events beyond $2\frac{1}{4}$, to the other as 1; and the same conclusion is drawn from a series of experiments made at Bristol, by Messrs Herapath and Rootsey, in whose results, Mr Peckstone has remarked, that every reliance may be placed, as they could not be actuated by party-feeling, but solely by a desire to ascertain the truth. These statements, so very discordant, must arise either from the defective mode of ascertaining the intensity of the light, or from the variable quality of the gas, both of which, I believe, but particularly the latter, have had their effect.

The mode usually followed for ascertaining the illuminating power, viz. of producing the same intensity of shadow, and marking the quantity of the gas consumed in a given time, is liable to many objections. It is extremely difficult, for instance, to judge with precision of the depth of shadow; besides, unless each gas is burned under circumstances favourable for producing the greatest light, the conclusion with respect to their power of illumination is not correct. Some of the experiments in which the oil-gas is stated as $3\frac{1}{2}$ to 1, it has been said, were conducted by using burners of equal dimensions for both: now, it is well known that the former requires a smaller one than the latter, otherwise the intensity of the light is not in proportion to the gas consumed, part of it probably escaping combustion. The remark, with respect to the variable quality of the gas, is also of equal force. In a paper published by Mr Dewey, in the Annals of Philosophy for December, some experiments on the illuminating power of the gases are stated, with a view of "setting the matter at rest." This I conceive they have done, as far as can be expected; but I suspect the conclusion to be drawn from them, is very different from that at which Mr Dewey arrives. The gases were taken from main-pipes running parallel to each

other, the coal-gas from the Imperial Gas Works, the other from that at Bow. Being burned so as to afford the same intensity of light, the quantities were found by accurate metres to be (taking the mean of seven trials) as 4850 to 1368, very nearly $3\frac{1}{2}$ to 1. Though these experiments seem to have been conducted with every degree of care, yet I am inclined to consider the coal-gas of very inferior quality. It has been supposed by some, that the specific gravity of coal-gas is a good test of its purity, the lighter it is the greater being its power of illumination. The experiments of Dr Henry, and others, however, disprove this; indeed, after the gases are properly purified, I believe the heavier they are, so much the more will be the light afforded by their combustion. The gas used by Mr Dewy was of specific gravity .406, now this is less than pure carburetted hydrogen, which is .555. In a note to the same paper the editor remarks, that the results of Mr Dewy coincide with those obtained by him and Mr Faraday. The coal-gas they subjected to trial was, in one instance, of specific gravity .429, in another, .406. The oil-gas was .965 and .939, and their illuminating power to the former was as $3\frac{1}{2}$ to 1. As the coal-gas in all of these experiments was of inferior specific gravity to carburetted hydrogen, we may reasonably infer, that they contained a considerable proportion of pure hydrogen, which, it is well known, affords a very feeble light. That I am correct in this assertion, at least that the coal-gases were of inferior quality, is proved by the experiments of Henry and others. Dr Henry has found the gas to vary in specific gravity from .345 to .650, its illuminating power increasing as it approached the maximum. The specific gravity of the coal-gas of Edinburgh, which is allowed by all to be of very superior quality, I have found to be so high as .680. The oil-gas used by Mr Dewy was .939. Dr Henry mentions, that in some of his experiments it was .906. I have found that, from the small apparatus of Mr Milne (Taylor and Martineau's), to be .940, and which is generally allowed to be very fine, so that we take it for granted, that that of Mr Dewy was of good quality. If, then, in these trials a good oil-gas, pitched against a very inferior coal-gas (which I think I have proved it to be), is only as $3\frac{1}{2}$ to 1, the illuminating power of the former must be much reduced when brought in competition

with the latter when of equally good quality; consequently, it must be far short of that stated by Mr Dewy.

Dr Henry, in his paper on the nature of the gases produced by the decomposition of coal and oil, proposes to ascertain their illuminating power by finding the quantity of oxygen necessary for their combustion; for, according to him, the more a gas will consume, the more light it will afford. He has found, that oil and coal gas, produced under different circumstances, take different quantities of oxygen.

	Specific Gravity,	took of	Oxygen
100 Volumes of Coal-Gas of	345		78
	500		166
	620		194
	630		196
	650		217
100 Volumes of Oil-Gas of	464		116
	590		178
	758		220
	906		260

From the above tables it would, of course, be inferred, that the illuminating power of oil-gas, No. 4., is the greatest, that of coal-gas, No. 1., the least, these being to each other as 260 to 78; that is, as $3\frac{1}{4}$ to 1. From this, then, it appears, that the *best oil-gas* is to the *worst coal-gas* as $3\frac{1}{4}$ to 1; of course a very different conclusion with respect to their illuminating power would be drawn, were we to take an average from the above tables, by which we should diminish the light given out by the former, and increase that from the latter.

There is still, I conceive, another mode of ascertaining the comparative illuminating power of the gases. It has been already stated, that the gas from oil and coal, the latter being freed from all impurities, is a mixture of variable proportions of olefiant gas, carburetted hydrogen, hydrogen, carbonic oxide, and azote, with an essential oil held in solution; the first of which is probably the principal source of light; the other combustible gases affording, by their combustion, very little. If this be the case, by finding its quantity we have an easy mode of ascertaining the relative illuminating power. The olefiant gas in coal and oil gas may be condensed by chlorine, provided the

mixture be excluded from light, to prevent any action on the carburetted hydrogen; hence affording an easy method of ascertaining its proportion. The mode of performing the experiment is very simple: A graduated jar, inverted on a water-trough, must be filled to the mark 50 with the gas, and 50 measures of chlorine are then to be introduced, the tube being covered with a paper shade to prevent any action on the other gases. In the course of from ten to fifteen minutes the condensation is completed. As chlorine and olefiant gas combine in equal proportions the diminution in the mixture will at once indicate the quantity of the latter in 100 parts of the gas subjected to trial; thus, if the water should rise to 40, the gas must have contained 40 per cent. of olefiant gas. From the trials I have made, I find that it promises to be a very easy, and, as far as I have found, an accurate mode of ascertaining the comparative illuminating power. As tried in this way, it is (with oil-gas and coal-gas prepared in Edinburgh), as 17 to 31; that is very nearly as 1 to 1.8.

Numerous other trials have been made on oil-gas with this test, and in none of them, except in one, did the quantity of gas condensed by chlorine come up to double that of coal-gas; but this gas was prepared by a small apparatus of my own, and is not therefore to be brought forward as a fair specimen. In some of the others the quantity did not exceed from 25 to 28.

If this method should ultimately prove correct, it will afford a mode, not only of finding the comparative illuminating power of gases when tried in the same place; but it will enable us to compare the light given out by one gas with that from any other, for we have merely to fix some point to commence with, and calling the illuminating power at this as 1, the others will bear a ratio to it, according to the quantity of olefiant gas they contain above that taken as the standard.

Though the different methods mentioned may afford a mode of ascertaining the comparative illuminating power of one specimen of coal-gas to that of a specimen of oil-gas, yet, I think it is impossible, from the experiments of one individual, to come to any general conclusion, the quality of the gas depending so much on the mode of manufacture. From the different experiments I have performed on coal-gas it seems to vary more

in its composition than oil-gas; and hence, also, the great difference in the statements of their illuminating power may have arisen, not from overrating that from oil, but from underrating that from coal, which is now, in general, of very superior quality. To come, therefore, to a conclusion with respect to their power of giving light, either the average of all the statements ought to be struck, or we ought to take best coal-gas, and put it in opposition to best oil-gas, as it must be the endeavour of different companies to adopt those means by which they may be obtained in greatest purity. If this be done, I suspect coal-gas will be found to possess, or at least may be made in general to possess, about half the illuminating power of that from coal. In Edinburgh, where the coal-gas is of very superior quality, I have found it, as I have already stated, by the chlorine test, to be nearly as 1 to 2, which I have confirmed by experiments, performed in the usual way, of producing the same light, and marking the quantity of gas consumed.

We come now to the important question, Can oil-gas, in point of economy, compete with coal-gas? and it is with a view of attempting to answer this, that I have dwelt so long on their comparative illuminating power.

There can be no doubt, that if oil gas could be afforded at as cheap a rate as coal-gas, it ought for many reasons to be preferred. Much less capital is necessary to establish an oil-gas work than one of coal; the premises required are less extensive; the gas-holders smaller, and there is no necessity for the expensive, and, unless properly managed, offensive purifying apparatus. Other circumstances would lead us to give it a decided preference; but, with all these advantages, unless it be as economical as coal-gas, it of course cannot come in competition with it. Two advantages, it has been often stated, must induce many to prefer it to coal-gas, its less unpleasant odour, and its being free from all noxious ingredients, such as sulphuretted hydrogen, which renders the coal-gas so injurious to silver and plated goods. With respect to the odour, from all the trials I have made, I must confess, that there seems to be very little difference; if any, it is certainly in favour of oil-gas; but this, I conceive, ought not to be brought forward as any advantage which the one may possess over the other, because, if all

the junctures of the pipes are tight, there ought to be no escape of gas; but, allowing that there is, it is fortunate, as I have before remarked, that it is possessed of odour, because it gives us warning of its escape.

That oil-gas does not contain any sulphuretted hydrogen, is allowed by all; but that it is from this to be preferred to coal-gas, must not be taken for granted; for though the gas given off from coal is at first loaded with sulphureous vapour, it is possible to free it entirely of it. Mr Neilson, Glasgow, in his report to the Dundee Company, states, that the gas made in the gas-works there is free from sulphuretted hydrogen, and does not tinge either silver or gilding. I have found the coal-gas of Edinburgh to be also free from it, as it did not discolour, in the slightest degree, paper dipt in a solution of sugar of lead, which it ought to have done, had it contained the smallest portion of it. It has been stated, that coal-gas has an advantage over oil-gas when required for street-lamps, because, as burners with very small holes are used for the latter, the flame is apt to be extinguished by wind; but this objection no longer exists, the small burners being laid aside for others, with holes a little larger, though not quite so much so as those used for coal-gas. Allowing, however, that the properties of oil-gas are such as to make us give it a preference, let us now inquire if it can be afforded as economically as that from coal. From what is already before the public, and perhaps the fullest account is to be found in the Dundee Report, it is impossible to come to any conclusion with respect to the profit likely to be derived from oil-gas establishments, they are so much at variance; but, luckily, there is no necessity for having recourse to these *calculations*. Indeed, in attempting to find whether oil-gas can compete with coal-gas, I would throw them entirely aside, for why should we trust to speculations, when we have facts deduced from experience, to lead us to the decision of the question?—I mean the expence of manufacturing the gases.

In endeavouring, then, to answer this question with respect to their comparative merits, there are two circumstances which ought to guide us, the comparative illuminating power of the gases, and the expence of making them. I have already stated, at full length, the various opinions with respect to

the difference in the light afforded by equal quantities of the gases, because, unless this be settled, it is impossible to determine whether oil-gas can compete with coal-gas, and the conclusion at which I arrived was, that, considering the superior quality of coal-gas, as now generally manufactured, we cannot expect it to be above oil-gas 2 to coal-gas 1; and in allowing this, I am confident I do it every manner of justice. Unless, therefore, oil-gas can be made and afforded at about double the price of coal-gas, it cannot come in competition with it. Though I have endeavoured to fix the illuminating power of the gases in general, yet this is perhaps not necessary in the question with respect to the comparative value, as this must depend, in a great measure, on the situation, because the price of the raw materials, and other circumstances, may make the cost-price of the gases to differ. It is enough, therefore, if we compare the oil-gas with the coal-gas manufactured at the same place, or if we take the coal-gas as manufactured where the materials can be got at an *average* price.

It is unnecessary to enter into any minute calculation with respect to the expence of generating coal-gas; according to Mr Neilson it is manufactured at Glasgow for 4s. 6d. per 1000 cubic feet, where, it is well known, it is made under very advantageous circumstances. By Mr Peckstone, it is estimated at 5s. 6d. In these statements we are not informed whether the loss by leakage, &c. is taken into calculation; if not, the expence ought to be considerably higher. At Edinburgh, coal-gas is manufactured for 4s. 11d. per 1000 cubit feet, but making allowances for the loss, it costs 8s., which we must consider, therefore, the cost-price of the gas; and it is sold at 12s.,—so that it affords a good interest on the capital, at present 8 per cent. Suppose, then, we consider the cost price of coal-gas as 8s. per 1000 cubit feet, the question is simply this, can oil-gas, allowing it to have double the illuminating power, be made for about 16s. per 1000 cubic feet, or can it even be made at 24s., and return a similar profit, for there are many who, having a high idea of its superior quality, will give three times the price of coal-gas. If it cannot be made at this, it cannot come in competition with it.

Mr Ricardo, who was a very strong advocate in favour of oil-gas, has stated its cost price to be at the least 27s. per 1000 cubic feet, taking the oil at 2s. per gallon. At Hull the same

quantity is made for 28s., and at Bow for 26s. Mr Peckstone has fixed the price at 27s. 9d., or say in whole numbers at 28s., and Messrs Taylor and Martineau, the patentees for the oil-gas apparatus, have stated it at 26s. Allowing, then, that we take the *lowest* of these, and it is the price given by the patentees themselves, oil-gas is manufactured at more than triple the price of that of coal-gas. If this, then, at the present rate of oil, (and we do not suppose that it can come much higher for palm oil, can be got at nearly the same price), be the lowest at which oil-gas can be made, and if we are right in fixing its illuminating power, compared with coal-gas, at about two to one, it never can come into competition with it; but suppose that it affords thrice the light, it ought, coal-gas being made at 8s., to be manufactured for 24s.; but the price is above this, so that giving it every advantage with respect to illuminating power,* it cannot at the price, at which it is stated to be made in general, compete with it. Suppose, however, that it is still maintained, that it possesses thrice the illuminating power of coal-gas, and could be manufactured at about three times the price, it ought to compete with it; and I conceive that this may be done in manufactories working under the most advantageous circumstances, as is the case at Leith. In this establishment the manager, with a salary of L. 100 a year, acts as gas-manager, book-keeper, engineer, and secretary, which situations are in some oil-gas companies filled by individuals having salaries each above that of the manager at Leith. Were an establishment of this nature in full employment, all the gas they could make being consumed, it may perhaps afford gas cheaper than that above stated, and which many, at least those who have a high idea of its properties, would pay for in preference to coal-gas. Six retorts, which the Leith Gas Company has at present, will make 8000 feet per day, working 10 hours. To make these, requires 80 gallons of oil at 1s. 6d., but say it should rise to 1s. 9d., for which, should it become

higher, palm oil can be purchased, is -	L. 7	0	0
Coals and Cinders, - - - - - -	0	8	0
Manager, - - - - -	0	6	0
Men's Wages, - - - - -	0	8	0
Tear and Wear (at 100 a year, a good allowance),	0	6	0
	L. 8	8	0

So that 8000 feet might be made at L. 8, 8s., but we must make allowance for loss before this is sent to the consumers, which is equal to about one-sixth *, reducing the quantity to 6600 feet, making the cost price 25s. 6d. Let us inquire, then, if made at this rate, and taking it for granted that the illuminating power is as three to one, consequently sold for thrice the price of coal-gas, it would afford a profit to share-holders.

Though the Leith Company has now only six Retorts, yet, in extending their works, they will require six more, and the capital is L. 15,000.

Twelve Retorts should make 16,000 feet per day; but if this be the greatest quantity that can be made, and necessary for winter consumption, the demand must be less in summer. Taking the number of hours the gas would be required during the year, the consumption would amount to very nearly 4,000,000 feet. The expence of making this, taking into calculation the loss by

leakage, &c. amounts to - - -	L. 5100
And sold at 36s. per 1000 feet, would yield -	7200
Leaving a surplus of - - - -	L. 2100

to pay the interest on L. 15,000, and lay by a sinking fund for contingencies.

But this is proceeding on the supposition, that, from its supposed illuminating power or good qualities, it would be purchased by many at three times the price, (I mean foot for foot), the oil-gas necessary for yielding a certain light, being only one-third of the quantity of that of coal-gas. If this is not done, then oil-gas cannot, *at the price at which it is now made*, compete with coal-gas.

It may be asked, if oil-gas can be manufactured at about 25s. 6d. per 1000 feet, why does a company, such as that at Leith, charge 40s., (and this is the lowest which has yet been charged)? To this the answer is obvious. They are not *now* making it at 25s. 6d. Every establishment of this nature is, at its commencement, not in full employment, and there is not therefore a sufficient income from the quantity of gas consumed at present, to meet the expenditure, and also to return a profit on the capital, supposing it sold at a lower rate. Hence from

* This is the actual loss, as ascertained by various trials.

this, arises another question. Were the company to reduce still farther their charges, would it increase the demand and consequent profit? There is no doubt it would increase the demand, for at present, owing to the high price, there is not much gas used, consequently part of the capital is lying dormant; whereas, were the price reduced, there might be a greater demand, which might enable the company to make the gas at a less price than they are now doing, consequently make the selling price less. But this would not operate to so great an extent as in coal-gas manufactories. In these the price of the raw materials is a trifle compared to the other expenditure, consequently by enlarging their work, the gas can be made at comparatively a less expence; but in oil-gas works, the chief expence is that of the oil, and therefore in making a larger quantity of gas, though not requiring much more to be laid out in labour, &c. the outlay for oil keeps pace with the additional quantity of gas required.

Notwithstanding I have thus given an opinion against oil-gas as now manufactured, I do not mean to assert that it never can come into competition with coal-gas. Oil-gas establishments are in their infancy, and, as it has been proved, that, by the present mode of decomposing oil, there is a considerable loss in the illuminating power by converting it into gas, other more effectual methods of decomposing it may be discovered, which will of course diminish the expence of the gas, allow it to be offered at a lower rate, and thus bring it into competition with coal-gas, *provided we consider it as having three times the illuminating power*; but if, as, I have endeavoured to prove, it is only about twice that of good coal-gas, I fear it never can come into competition with it.

I cannot conclude these remarks, without publicly expressing my thanks to Mr Watson, manager of the Edinburgh Coal-gas Company, and Mr Forrest of the Leith Oil-gas Works, for the information they have given me, on the expenditure and income of their establishment; and for the assistance they have afforded me in enabling me to conduct the few experiments I have stated on the quality of the gases.

Art. XX.—*Remarks on the subjects connected with Dr* Brewster's *"Reply to Mr* Brooke's *Observations on the Optical System of Mineralogy."* By H. J. Brooke, F.R.S.

Dr Brewster has published in No. 18. of this Journal "a Reply" to what he terms my "observations on the connexion between the *optical structure* of minerals and their *primitive forms*." Dr Brewster must allow me to say, that, as I disapprove the phrases *optical structure* and *primitive forms*, I have not used either of them, except in quoting a paragraph of his.

Dr Brewster states, that, unwilling to occupy his time with such discussion, he would have addressed me privately on the subject, if the learned Editor of the Annals of Philosophy had not inserted my observations in that work. On this matter, I have only to remark, that I was ignorant even of the learned editor's intention of giving any account of my volume, until after the number which contained his notice was published.

As a matter of mere feeling, I can assure Dr Brewster, that an object of greater indifference scarcely exists, than whether *Tesselite* be or be not *Apophyllite*, or whether the present systems of mineralogy shall or shall not be superseded by Dr Brewster's optical system. I cannot, however, suffer Dr Brewster's hasty "*reply*" to pass unnoticed.

The reader who has perused this "*reply*," will have seen that the opinions which have operated so powerfully upon the feelings of Dr Brewster, relate to these points.

1. Whether the mineral which Dr Brewster has named *Tesselite*, be really a distinct species from the *Apophyllite*, considered either chemically or crystallographically.

2. Whether the crystalline form of the sulphato-tri-carbonate of lead be a rhomboid.

3. Whether the use of optical characters can at present be relied upon for the determination of a mineral species.

Before I proceed to justify the opinions I have expressed on these points, it will be necessary to consider for a moment the meaning of the term *primitive form*, and what Dr Brewster's ideas are of a *mineral species*.

A *primary form* of a crystal is a simple figure, bearing certain known and demonstrable relations to other figures, belong-

ing to the same species of mineral, which are termed *secondary*.

But a *primitive form*, according to Dr Brewster's use of the term, seems to be something much less definite.

It is true, that in p. 12. Vol. VII. of this Journal, Dr Brewster alludes to the "method of crystallographic analysis, by which we ascertain the mechanical structure of crystals by cleavage, and thus obtain for each mineral species *a primitive form*, to which all its secondary forms may be referred." I have, however, endeavoured to shew, in the volume on crystallography referred to by Dr Brewster, that cleavage does not perform this crystallographic analysis, except in particular instances. For we can frequently obtain by cleavage from the same crystals several forms, to any of which its secondary forms may be referred; and hence we may be said to obtain *several primitive forms* from one crystal. But the supposed *primitive forms* of Tesselite do not appear to have any relation even to cleavage, nor, as we shall presently observe, is it very evident what Dr Brewster's notion of *primitive form* is, in reference to that substance.

What Dr Brewster understands by a *mineral species*, is also very indistinctly seen in his papers. Berzelius considers all minerals which are composed of *similar elementary particles, united in equal proportions*, to belong to one *species*. Haüy adds to this chemical character the crystallographic condition, that these elements shall be combined into *molecules which are similar in form in all the individuals belonging to the species*.

Perhaps Dr Brewster will either add to this definition, or substitute for it, a condition, that the minerals comprised in one species shall all resemble each other in their *optical properties*. This, at least, is what we might expect in an *optical system of mineralogy*. But a crystal of Tesselite would, according to either of these definitions, belong to several species.

It would appear, however, from Dr Brewster's experiments on glass, which will be afterwards more particularly alluded to, that the optical character of every known crystal may be imitated in that substance. For, Dr Brewster asserts in p. 260. of the *Phil. Trans.* for 1818, that there is not one phenomenon belonging to regular crystals which he has not been able to imitate in glass. If optical phenomena, therefore, are to constitute *specific characters* among minerals, does it not follow, either that

glass changes its species with its change of optical character, or that optical characters are insufficient to distinguish species? What, therefore, does Dr Brewster mean by a Mineral Species?

The phrase *optical structure*, as used by Dr Brewster, does not appear to relate properly to *structure*, but is used to express *some property of matter which is manifested by it on light, and of which it is probable that as little is known as of the nature of light itself.*

The term *structure* appears, indeed, to be used in several senses by Dr Brewster. In the *Phil. Trans.* for 1818, p. 158. the *structure* of glass is said to be *altered by pressure or dilatation.*

It is also stated, that, in glass, the *polarising structure* "depends *entirely* on the *external form* of the plate, and on the mode of aggregation of its particles." The dependence of *structure* upon *external form* is not immediately obvious.

Before we examine the chemical and crystallographical evidences of the identity of the *tesselite* and the *apophyllite*, let us see what Dr Brewster's own opinions have been concerning the *tesselite.* A paper relating to this mineral was published in 1819, in the first volume of this Journal, which Dr Brewster, on another occasion, says contains an account of his *first experiments* on this remarkable substance. In p. 1. of that volume it is stated, that "the *apophyllites* from Faroe crystallise in *quandrangular prisms*, with *flat summits*, having *a truncation upon the angles*; and also in *single and double four-sided pyramids*, the planes of which correspond with the small truncations upon the prisms, and form angles of 60° * with those on the opposite side."

Dr Brewster says in p. 2, "Having cut off the slice, about $\frac{1}{20}$th of an inch thick, which formed the summit of a quadrangular prism, I found that it had only *one axis* of *double refraction*, and *produced the same set of coloured rings* as *apophyllite* from *Fassa*; but upon removing a *second slice*, of the same size, it exhibited the appearance of a *tesselated pavement* †, composed of *four rectangles*, surrounded by a *border*, and having in the *centre another rectangle*, with its sides opposite the angles *of the quadrangular prism.*" If the reader will turn to Fig. 1. Plate I. of the same volume, he will find the figure which illustrates this description. In p. 3. it is said, "This singular con-

* This angle is 59° 16′ very nearly.

† Hence the name 'Tesselite.'

struction has no resemblance to that of *macled* or *hemitrope crystals for the four rectangles* are portions of *one crystal;*" which is very evident, supposing the figure to be correct. But it is said in p. 5, "The *interior* conformation of apophyllite [tesselite] presents us with the *new fact in* crystallography, that a *regular crystalline form* arises in some cases from the union of smaller crystals, *whose homologous sides are not parallel to each other.*" The *four rectangles* are *portions of one crystal* in the first paragraph, and in the second they are *smaller crystals*, whose *homologous sides are not parallel to each other.* How are those opinions to be reconciled? But it is unnecessary to discuss this point, as Dr Brewster has, in a memoir inserted in the Edinburgh Philosophical Transactions for 1823, *virtually abandoned this early view of the rectangular structure of the crystals of tesselite*, and has substituted for it a *new* theory of their formation, *not less extraordinary than the phenomena from which it has been inferred**. Dr Brewster there says, that the tesselite *could not have been formed* by the ordinary process of crystallisation, but that "*a foundation appears to be first laid by means of uniform homogeneous plates, the primitive form of which is pyramidal; a central pillar, whose section is a rectangular lozenge, then rises perpendicularly from the base, and consists of similar particles. Round this pillar are placed new materials, in the form of four trapezoidal solids, the primitive form of whose particles is prismatic, and in those solids the lines of similar properties are at right angles to each other. The crystal is then made quadrangular by the application of four triangular prisms of unusual acuteness. These nine solids, arranged in this symmetrical manner, and joined by transparent lines performing the functions of a cement, are then surrounded by a wall composed of numerous films, deposited in succession, and the whole of this singular assemblage is finally roofed in by a plate exactly similar to that which formed its foundation!*"

Ample as the preceding remarkable description may appear, it is yet deficient in several important particulars. What is the *figure* of the *solid* which Dr Brewster calls the *primitive form*

* This memoir is said to have been read in 1817 and 1821. How has it happened that Dr B. should have suffered so curious a discovery to sleep so many years in the archives of the Society?

of the *foundation-stone*, the *roof*, and the *central pillar?—and*, at *what angle do its planes incline to each other?* What are the *plane angles* of the *section*, or *imaginary base*, of the *four trapezoidal solids?* Are these angles *constant* in the *same crystals*, and in *different crystals;* and are they constant throughout the *whole length* of these *imaginary forms?* What is the *figure*, and what are the *angles*, of the *primitive form* of the *particles* of which the *trapezoidal solids* are composed? Are the *triangular prisms* composed of *particles similar to these?*—and *how are they arranged* in the *construction of each of these two sets of prisms?* When information is afforded to the crystallographer on these points, he will be enabled to ascertain whether the crystals fall within the range of ordinary laws of structure or not.

But we may regard the necessity of these inquiries to be nearly superseded by Dr Brewster's *latest* opinion relative to this mineral, as it appears in p. 364. of No. 18. of this Journal. In looking through the different papers in which Dr Brewster has alluded to the *tesselite*, it is not easy to collect whether he attributes its extraordinary optical character to its *structure*, or to the *nature of its elements*. The quotations I have just given would seem to refer the optical phenomena principally to its *structure*, but in the following passage they are more particularly attributed to the *properties of its elements*. In this passage, it is stated, that "*tesselite is a substance built up, as it were, of the most singular elementary parts, all of which parts have different optical and mechanical properties.*" A climax, indeed, in the description of this mineral, which transcends all Dr Brewster's previous views regarding it.

"Hence," he says, "an analysis of tesselite resembles the analysis of a bird, the feathers, flesh and bones of which are all pounded together in a mortar." But if this last description of the substance were correct, and the mortar were to contain the whole of the animal, vegetable, and mineral kingdoms together, the mass would be homogeneous in comparison with *tesselite*.

Let us, however, quit these regions of imagination, and examine the evidences of the chemical distinction of *tesselite* from *apophyllite*.

In page 364. of No. XVIII. of this Journal, Dr Brewster quotes a paragraph of mine, stating, *that* "*Tesselite* does not,

chemically, appear a *distinct species of mineral;*" and he follows up his quotation with this remark: "The conclusion, as now stated by Mr Brooke, *I must positively deny.*" A reference to some facts might surely have been expected from Dr Brewster, in support of this denial; but not a syllable of proof follows. I shall, therefore, attempt to supply those evidences which Dr Brewster has omitted.

In considering the chemical character of the apophyllites, it will be necessary to recur to the two papers on this subject, commencing at page 1. of the 7th volume of this Journal. On referring to the first of these, it will be seen that Sir George Mackenzie had sent to Berzelius some specimens of the mineral named *Tesselite* by Dr Brewster; and that these were, at the desire of Dr Brewster, analysed by Berzelius. This gentleman also analysed specimens of the *Apophyllite* from Uto, and found their composition similar to that of the *Tesselite.* In page 18., Dr Brewster says, "I have *reason to believe,* that the *Tesselite* exists among the *Apophyllite of Uto;* and *therefore it is not improbable that Berzelius may have analysed the Tesselite from Uto.*" Would it not have been more satisfactory to his readers, if Dr Brewster had assigned the reason which induced him to believe that his Tesselite had been found at Uto? The tendency of Dr Brewster's conjecture was to rid himself of a fact unpropitious to his theory; and it therefore became more incumbent on his candour to have fully stated the grounds of his belief.

Dr Brewster goes on to say, that "the two kinds of apophyllite which are set in peculiar contrast with one another in the optical method, are the Apophyllite surcomposé of Fassa, and the Tesselite of Faroe; and therefore the analysis of these two crystals, *when the former is perfectly pure,* should be well compared." Dr Brewster probably was not aware of the analysis of the *Tyrol Apophyllite,* by Gmelin, or he might have himself made the comparison he recommends; and he would then have found the Tyrol mineral (but of what form the crystals are, is not stated), agreeing as closely in its composition with the Faroe variety, as the Uto specimens had done.

The results of the analysis of the three varieties are as follows:

			Silex.	*Lime.*	*Potash.*	*Fl. Ac.*	*Water.*
Tesselite,	Faroe,	Berzelius,	52.38	24.98	5.27	0,64	16.20
Apophyllite,	Uto,	Berzelius,	52.19	24.71	5.27	0,82	16.20
	Tyrol,	Gmelin,	52.38	24.86	5.27	—	16,19

From this statement, the reader will judge how far I am justified in concluding, that, "*chemically, the Tesselite does not appear a distinct species;*" and to what confidence Dr Brewster's *positive denial* of the accuracy of this conclusion is entitled.

I have re-examined, with some attention, the crystallographical characters of the apophyllites, and I must confess myself unable to discover in what *essential* particulars the tesselite differs from the others. If astonishment at the splendour of the *optical imagery* of tesselite had not interfered with Dr Brewster's observation of other matters, he might have perceived, that *the transverse cleavages were all perpendicular to the axis of the prism, and were consequently parallel to each other.* If the crystals had been composed of *pyramidal primitive* forms in their interior, and *prismatic primitive forms* surrounding these, it is not probable that they would all have lain in precisely the same planes. But if Dr Brewster had looked a little closer to the laminæ obtained from cleavage, he might possibly have perceived that his *wall and his nine prisms, were all traversed by indications of continuous natural joints, parallel to the edges and diagonals of the plate;* for such joints are very apparent in several of the slices I have examined, and which contained outlines analogous to those of Dr Brewster's trapezoidal solids. These outlines do not, however, appear to *be quite as regular as* Dr Brewster has drawn them; nor do they seem to be always referable to precisely the same cause; for, upon breaking several crystals in which they were visible, I have observed that they were the edges of planes of composition, which were sometimes parallel to the lateral planes of the prism, and were sometimes oblique,—those parallel to the lateral planes having their surfaces striated longitudinally, and the oblique planes being obliquely striated. It seems probable, that the *outlines* supposed to belong to the four rectangular solids, might result from the transition of a *pyramidal* to a *prismatic* crystal *; or rather, perhaps, from the joint action of the two forces or circumstances, be they

* This is a fact of frequent occurrence among crystals, particularly in carbonate of lime, where regularly-formed figures are enveloped by new deposites upon their

whatever they may, which would singly have produced theperfect pyramid, or the perfect prism; and his trapezoidal outlines may also be referred to an analogous cause. Dr Brewster allows that some of the Faroe crystals have not the tesselated structure,—a fact which is not at variance with the preceding conjecture.

I have just measured several crystals of the apophyllites from Faroe, Uto, and Fassa, and have found them agreeing remarkably in the inclinations of their corresponding pyramidal planes.

If we denote the two adjacent pyramidal planes of Dr Brewster, Fig. 7. Pl. I. Vol. I. of this Journal, by a and a', the square terminal plane by P, the measurement may be given thus:

	Faroe.		Uto.	Fassa.
a on a' —	104° 6 to 8′		104° 10′	104° 7′ to 10′
a on P, is consequently		-	119 38 nearly.	
a over P, on a,	-	-	59 16	

I can feel no hesitation, therefore, in saying, that the *Tesselite* agrees, crystallographically, with the *Apophyllites* from Uto and Fassa; and, as we have seen that its chemical composition is also similar, it cannot, I think, be ranked as a distinct species.

On the crystalline form of the *Sulphato-tri-carbonate of Lead*, I have nothing to offer in addition to what I have stated in a separate article in the present Number of this Journal.

I may refer here to a circumstance alluded to in p. 370. No. XVIII. of this Journal. Dr Brewster says, " The minerals of the Zeolite Family having been *particularly examined* by Mr Brooke and myself, we have had occasion to apply our respective methods of observation to several minerals which had not been carefully studied by preceding mineralogists. Two of these minerals were the *Nadelstein* of *Faroe*, and a *mineral from Aix-la-Chapelle, supposed by* Haüy to be a Stillbite. *All the resources of crystallography in the hands of Mr Brooke were unable to* distinguish these minerals from those already known. *I* have determined both of those to be new minerals; and the accuracy of these determinations has been *completely established by chemical examination.*"

The part of this quotation which relates to me is altogether without foundation. The only specimens of the *Faroe nadel-*

surfaces, and where the external form of the latter deposites differs from that of the included crystal.

stein, which I have ever been possessed of, were a few fragments of crystals without summits, and with very imperfect natural planes, for which I am indebted to Dr Brewster. All I could do with these was to cleave and measure them. And on applying the minute fragments obtained from cleavage, to the reflective goniometer, the inclination of the cleavage planes appeared to be the same as that of the common *needlestone*.

The case of the mineral from Aix-la-Chapelle, now *Hopeite*, is different. On turning to a letter of Dr Brewster's, I perceive, that our last communication relative to this mineral was in December 1821. At that time I had seen only one crystal of it, which was in *a deep and narrow cavity of the matrix, and presented only one of its sides to the observer*. This, I believe, I mentioned to Dr Brewster at that time. In such a state, it is plain it could not be *particularly examined*. Some months afterwards, my friend Mr Heuland gave me a detached crystal of this substance, and all the mighty resources which were called forth for its examination, were the measurement of two angles by the goniometer, and the resolution of a single spherical triangle. The result gave a difference from the Stilbite in the dimensions of the prism, supposing it to be rectangular, and in the inclination of its lateral planes, supposing it to be rhombic; and this was communicated to Mr Heuland a few days after I received the crystal. I should not have taken the trouble of relating these very unimportant circumstances, but for the purpose of correcting Dr Brewster's inaccurate statement.

Dr Brewster alludes to a chemical examination of these minerals, which, he says, has completely established his determinations. May I request him to state the *precise result* of this examination, and when, and by whom, it was made, as I cannot find any analysis of either of them published.

Having so far disposed of the preceding points, I have now only to offer a few remarks upon the *present state* of Dr Brewster's *Optical System*. The double refraction of crystals had been ascertained to belong to many substances long before Dr Brewster's researches began; but Dr Brewster ascertained its existence in many others; and that it might, in numerous instances, be observed in more than one position of the crystal; and the term *axis* of double refraction, has been introduced to

express an *imaginary line*, bearing a certain relation to the *position* in which this property may be observed.

Dr Brewster complains, that, in my former allusion to his theory, " the reader is not told what the system is that is assailed ; he is not made aware that it has any foundation at all as a general principle ; he is not told of its *successive triumphs over everypositive objection that has been urged against it* ; but he is hurried at once, and without ceremony, to witness its demolition."

I shall now endeavour to atone for my former negligence, by presenting an outline of Dr Brewster's Optical System to the reader, and then stating those objections to it, *over which it has not yet triumphed.*

But it will first be convenient to insert an outline of Professor Mohs's Arrangement of Crystalline Forms, with which Dr Brewster states that he has found the optical classification very gene- to agree.

Mohs has separated what I have termed the *primary forms* of crystals, into *four systems* of primitive forms.

1*st*, The Rhomboidal,......Comprehending the *rhomboid*, and the *hexagonal prism*.

2*d*, The Pyramidal, ...Comprising the *octahedron with a square base*, and *the right square prism*.

3*d*, The Prismatic,.........Comprising the following prisms:
the *right rectangular*,
the *right rhombic*,
the *right oblique-angled*,
the *oblique rhombic*,
the *doubly oblique*,
And also comprising
the *octahedron with a rectangular base*,
——————— *rhombic base*.

4*th*, The Tessular,..........Comprising the
cube,
regular tetrahedron,
regular octahedron,
rhombic dodecahedron.

To which Dr Brewster has proposed to add the *composite system;*—an addition to which there can be little exception, as a matter of amusement, if it be established merely to receive those specimens, which, from the superior beauty of their optical phenomena, may claim a distinction in rank from their *Doric*, *Ionic*, and *Corinthian* neighbours. But it does not at present appear necessary for crystallographical purposes.

Dr Brewster appears to have examined a very considerable number of mineral and other bodies, by means of polarized light, and he has been led by this examination to the following optical classification of crystals: 1*st*, Those in which only *one axis* of double refraction has been yet observed; which include nearly all the crystals comprised in the *first* and *second* of Mohs's Systems.

2*d*, Those which possess *two axes*, consisting of nearly, if not entirely, the whole of Mohs's third system. But some which crystallographically belong to the first and second systems, are also included by Dr Brewster in this.

3*d*, Those in which no double refraction has been yet discovered, either by direct experiment, or by means of polarised light, and which belong to the fourth system of Mohs.

Out of these facts, which, we must allow, are very numerous, and hence very imposing, Dr Brewster has produced two optical systems; one relating to mineralogy generally; and the other relating to compounds of sulphuric and tartaric acids, with earthy, alkaline, and metallic bases, all of which are said to have *two or more axes of double refraction*. It will appear afterwards, that one or the other of these systems must probably be surrendered to a salt of nickel.

The assumed powers of Dr Brewster's optical system of mineralogy are very remarkable. We have already seen the *facility* with which it professes to unfold the *chemical* and *architectural splendours of tesselite*. In page 363. of No. XVIII. of this Journal, Dr Brewster assures us, that the *optical phenomena on which this system is founded*, are the *necessary results of a mechanical structure;* and that *their indications* must *infallibly harmonise* with *the sound deductions of crystallography*. Yet in page 364, Dr Brewster asserts, that *the tesselite possesses a structure which defies all the laws of crystallography*. It is not easy to perceive how *phenomena* which *necessarily result* from

a structure that *defies all the laws of crystallography*, must *infallibly harmonise with the sound deductions of that science.*

In page 14. of Vol. VII. Dr Brewster, in alluding to the powers of *optical analysis*, says, "*We* examine the mineral in its *perfect* and *undisturbed* condition; *we determine* its *various properties as modified by the elements of which it is composed*, by the *proportions* in *which they combine*, and by the *mechanical* or crystallographic structure into which they are *arranged: we therefore determine properties*, and *measure actions*, and *observe structures*, which *vary* with the *elementary parts of the mineral*, as well as with their mode of *combination*." This *conclusion* will certainly not be denied to Dr Brewster, by those who admit his *nearly identical premises*. A few lines farther on, Dr Brewster adds, "*In proof of these positions*, I might refer in general to the *universality of the law* which *I have established* between the *primitive forms* of crystals, and the number of their axes of double refraction."

I might dismiss these passages at once by observing, that they are hypothetical from beginning to end. I shall, however, make them the subject of one or two remarks. It is not very clear how the *observed relation* between the *primitive forms of crystals*, and the number of their axes of double refraction, should *enable the observer* to determine all the *various properties* alluded to in the preceding extracts. In the first of these, it will be also seen, that Dr Brewster says, "we examine the mineral in its *perfect* and *undisturbed* condition." It may, I think, be inferred from the context, that the term *perfect* here means *pure, unmixed with* foreign matter. But where is a specimen of this nature to be found for examination? And how many experiments has Dr Brewster made on perfectly pure and uncontaminated minerals? If the term *perfect* is not to be understood in this sense, but is taken to imply no more than the actually entire state of the crystal which is examined, be its impurities and composition what they may, the generality of any law, derived from the application of optical analysis, to any number of such specimens, might surely be received with some degree of distrust; as we may perhaps reasonably entertain a suspicion, that the results of optical analysis are liable to be influenced by the accidental presence of foreign matter in a crystal. It affords me some satisfaction to find that

I do not stand alone in opinion upon this point. In Vol. VII. p. 4. of this Journal, Berzelius says, "It appears to me, that mineralogical characters, drawn from optical phenomena, presented by transparent crystallised minerals, may be denied from several sources, 1*st*, From the nature of their elements; 2*d*, From the number of atoms of each element, on which depends the crystalline system to which the mineral belongs; 3*d*, From accidental mixtures, often inconsiderable, which alter, in an essential manner, the transparency, the forms, and the crystallisation. These are what are called, in artificial crystals, impurities. To these we may add the variable transparency of different parts of the same crystal. Differences of this kind, however great be the influence which they exercise upon light, can never constitute differences of species in mineralogy,—differences which can only be founded on a real diversity of composition. To distinguish among the optical phenomena produced by accidental circumstances, and those which are derived from a difference in the elements, or in the number of their atoms, is to carry to its maximum the employment of optical phenomena as distinctive characters in mineralogy."

Upon this Dr Brewster has the following observations in page 17. of the same volume: "Mr Berzelius has remarked, that while the optical characters of minerals may be derived from the nature of their elements, and the number of atoms of each element, they may arise also from *casual mixtures and impurities*, and that to employ *optical phenomena, produced by accidental circumstances*, is to *carry the use of this method to its maximum*. In this opinion we heartily concur; but we are not aware that any such optical phenomena have ever been proposed, even as subsidiary characters; and we should think little of any system that founded its decisions on the *variations of colours or of transparency*, or on *numerous other accidents of light which are familiar to mineralogists*. The system of optical analysis disclaims all such trivial distinctions, and founds all its results upon characters *as essentially necessary to the existence of the mineral, as the most prominent of its chemical elements*."

This, however, takes the question at issue for granted, and is any thing but an answer to the objections raised by Berzelius,

which evidently did not relate merely to *variations of colour or of transparency, or of other accidents of light which are familiar to mineralogists*, but to *that class of optical phenomena which had induced Dr Brewster to separate tesselite from the apophyllites, and, therefore, to Dr Brewster's Optical System generally.*

The salt of Nickel, already alluded to, is one of the Sulphates described in the *Annals of Philosophy* for December 1823. The form of this salt is a *square prism*, and hence ought to possess only *one axis* of double refraction, according to one of Dr Brewster's *optical systems*; but, according to his theory, which assigns *two axes* to all the *sulphates*, it should possess *two axes*. When Dr Brewster has examined the crystals of this salt which have been sent him by Mr Cooper, he will be enabled to state whether its optical character be at variance with any of his theories, and, if so, whether one of them must not give way.

Another fact which impugns Dr Brewster's optical system, relates to the Ferro-prussiate of potash. This salt was examined both by Mr Levy and myself, and found, by goniometrical measurement, to have for its *primary form* an *octahedron with a square base*, which, on reference to Mohs's classification of crystals, will be found to belong to his *pyramidal system*. Dr Brewster states, in No. XVIII. of this Journal, p. 371., that the *optical method* places this salt in the *prismatic system*, and he adds, "Mr Levy will, *we have no doubt*, see reason to correct his determination."

Dr Brewster has, however, rendered this correction unnecessary, by the discovery that some of the crystals possess *one*, and others *two* axes of double refraction.

The fact is thus announced, in a letter to Mr Cooper. "Perhaps you could explain to me a fact regarding prussiate of potash, which is rather perplexing. The *large* crystals, which are generally very compound in their structure, have, for their *primitive form* the *octahedron with a square base*; but when they are recrystallized, the most perfect of the *small* crystals belong to the *prismatic series*, and have *two axes* of double refraction." On this discovery being announced, I dissolved a portion of a very large crystal of this substance which had been previously

measured in distilled water, and have found the small ones produced from it correspond in measurement with the large crystal. The large and small crystals are therefore similar in external form.

In the Phil. Trans. for 1816, Dr Brewster announces the discovery, that *muriate of soda*, *fluor-spar*, and *diamond*, which belong to the *Tessular system* of Mohs, and hence ought to possess *no double refraction*, may receive that property by *compression or dilatation*. And in the volumes for 1816 and 1818, it is stated, *that all the properties of regular crystals might be communicated permanently to glass, and other crystallized bodies, by heat.*

The experiments made by Sir Humphry Davy, on the state of water and aëriform matter, in cavities found in certain crystals, and detailed in a communication printed in the Phil. Trans. for 1822, afford some ground to believe that crystals may sometimes be formed under different states of both pressure and temperature: And hence, judging from the experiments just referred to, on fluor-spar, glass, &c., we may conceive, that minerals which are *chemically similar*, but crystallized under different circumstances, may exhibit considerable differences in their optical phenomena, without possessing any other specific distinction. But Dr Brewster's experiments upon glass, and upon *muriate of soda*, *fluor-spar*, *diamond*, &c. appear to affect the very foundation of his system, which professes to distinguish the *primitive forms* of crystals by their *optical phænomena*. Glass, in its ordinary state, is denominated by Dr Brewster *an uncrystallized body*, and consequently can have no *primitive form*, in any sense of that term; but, by heat, he says it becomes *crystallized*, and may be impressed *permanently* with the doubly refractive property of crystals, so as to possess *one* or *two* axes of double refraction. Does not *the system* require us to admit, that it acquires at the same time the *primitive forms* of the crystals whose *optical phenomena* it is made to represent. For otherwise the *certainty* of the indication of *primitive forms* by *optical phænomena* must be abandoned. According to Dr Brewster's system, we must also suppose that *muriate of soda*, *fluor-spar*, *the diamond*, &c. *acquire* by *pressure* or *dilatation new primitive forms*, with their *new optical character*.

But the circumstance most likely to embarrass the mineralogist, even if his road through Dr Brewster's system were clear in every other respect, is the utter confusion of forms which one branch of Dr Brewster's theory would establish among crystals. "If," says Dr Brewster, "we take a crystal *bounded* by *six equal square faces*, the *crystallographer* will content himself with calling it a *cube*; but the *optical mineralogist* will only call it it a *cube* when it has *no double refraction*; he will *maintain it* to be a rhomb (*qu. rhomboid*), if it has *a single axis of double refraction, coincident with one of its diagonals*; and he will *consider it* as *a right prism with a square base*, if it has *a single axis of double refraction, perpendicular to any two of its faces*."

To this Dr Brewster will allow me to add, *and he will maintain, that it belongs to the prismatic system of Mohs, if it has two axes of double refraction.*

Thus, *each of the classes of the primary forms might, in its turn, be personated by the cube*;—a metamorphosis not very consistent with the notion of form, upon which crystallographical distinction has been hitherto founded. Let this difficulty, however, be overcome, and let the mineralogist be contented to talk of *a rhombic* or *doubly oblique prism*, *contained within six square planes*, still he will find, that in resorting to his *polarising mirrors*, he has not substituted for his *goniometer* an instrument capable of affording a greatly increased facility to his researches. For Dr Brewster has shewn that his optical method presents obstructions to the mineralogist, which would sometimes demand extraordinary perseverance and good fortune for the detection of a single species. He says, in p. 218. of the Phil. Trans. for 1818, "The *extreme difficulty* which attends experiments of this kind will be understood from the fact, that I cut *more than fifteen plates* out of a *large piece of zircon without discovering its axis*. By a *singular accident*, however, Mr Morton, a jeweller in Edinburgh, procured for me no less than *sixty plates of zircon* with *parallel faces*, and it was only in *two of these* that the system of rings was developed." But what is the mineralogist to do who is not in the way of such another singular accident?

Under all these circumstances, I may appeal again to the

reader's decision, whether I am not justified in the opinion I have expressed, that the connection between the optical characters of minerals and their crystalline forms, is not yet sufficiently understood.

Art. XXI.—*Some Account of the School of Arts of Edinburgh.*

The public have heard a great deal during the last year of institutions being established in different parts of the island, for the diffusion of scientific knowledge among operative mechanics, and several periodical publications in a cheap form, have of late appeared with the same laudable purpose. It is an important epoch in the history of the country, and a spirit has been awakened, which, if properly directed, may be productive of the most beneficial effects, not only by ameliorating the moral condition of a great mass of the people, but also by increasing the skill and prosperity of our various branches of manufacture. We are of opinion, therefore, that a brief account of the School of Arts of Edinburgh, which has now existed for three years, will be acceptable to our readers.

It is to Dr Birkbeck that the merit is unquestionably due, of having first proposed a plan for conveying scientific instruction to mechanics. While he was resident in Glasgow, and held the situation of Lecturer in the Andersonian Institution, about the year 1801, he gave lectures on Chemistry and Natural Philosophy to operative mechanics on certain days set apart for that purpose, a plan which was followed up by his successors, and has been considerably extended and improved by Dr Ure, who now delivers the lectures in that establishment. The valuable suggestions which Dr Birkbeck had thrown out remained unheeded for twenty years; for we believe, that, during the whole of that time, the lectureship which he had founded, was the only thing of the sort that existed in the united kingdom.

About the autumn of 1820, Mr Leonard Horner proposed the establishment of a school in Edinburgh, in which such branches of science as would be useful to mechanics in the exercise of their trade. might be taught at convenient hours, and at an expence

that would be within their reach, upon a plan similar to that of the Glasgow Institution. Having communicated his design to Dr Brewster, who entered warmly into his views, a sketch of a plan was drawn up, such as was thought probable might be carried into effect; and it was circulated among some of the most considerable master mechanics, with a request that they would read it in their workshops, and take down the names of such of their workmen as expressed a desire to obtain instruction of the kind proposed. This was accordingly done, and in the course of a fortnight between 70 and 80 names were put down. A Committee of several scientific gentlemen, and of master mechanics, was immediately formed for bringing the scheme before the public; it met with very general approbation, and a liberal subscription having been raised, a regular association was formed, under the title of "The School of Arts, for the instruction of Mechanics, in such branches of physical science as are of practical application in their several trades." The Institution was opened in October 1821, each student paying 15s. for a ticket, which entitled him to attend all the lectures, and have the use of the library for a year. Such was the eagerness for admission, that after 420 tickets were sold, the book was obliged to be closed, as the room could not accommodate a greater number. The lectures delivered the first year, were on the principles of Chemistry and their application to the arts, on the elementary principles [illegible] al Philosophy, on Architecture, and on Farriery. [illegible] of these lectures at the end of April, one lecture a week on each subject having been delivered during the preceding seven months, there was established a class for Architectural and Mechanical Drawing, which continued for four months. There was established also an excellent library, containing nearly 500 volumes of the best elementary works on the sciences taught in the school, together with some of the works of greatest authority in the mechanical arts. These books the students have the privilege of taking to their own houses, and they may be exchanged once a fortnight. The first session terminated very successfully; and it appears from the First Report of the Directors, that the students followed the lectures with the most profound attention and the deepest interest. It is stated in that report, that "no audience accus-

tomed to observe all those restraints which are the marks of good breeding and good education in the more elevated ranks of society, could have conducted themselves with more perfect propriety and decorum."

The following year the plan of instruction was in some degree changed, it having been found that the attention of the students had been too much distracted by variety; and that, in order to make the institution really useful, by conveying solid instruction to the mechanics, it was necessary to direct their whole attention to the acquisition of the elementary principles of chemistry and mechanical philosophy; as these were quite sufficient for all the time they could bestow, and were the branches of science of most general application in the mechanical arts. The Directors obtained also this year the powerful assistance of Professor Leslie in digesting their plan of instruction; and upon his suggestion a class was established for the higher branches of Arithmetic, and the elements of Algebra and Geometry, with their application. Without this instruction it was obvious, that the students could derive very little benefit from the lectures on Mechanical Philosophy, and what was of still more importance, without the knowledge of Algebra they could make no use of some of the most valuable works in the library.

The course of instruction during the second year was, a lecture once a week upon Chemistry, once a week upon Mechanical Philosophy, and the Mathematical Class met twice a week. This plan has also been followed during the last year, and the Drawing Class and Library continue on the same plan as the first year.

The Institution continues to gain ground in the estimation of those for whose benefit it was established, and it has now assumed all the characters of a regular seminary of instruction.

It is supported by the fees of the students, and an annual subscription among the inhabitants of Edinburgh, and other friends of the institution. The management of its affairs is conducted by Eighteen Directors, chosen annually at a General Meeting of the Subscribers. Many of the details are managed by Committees of the students themselves, appointed by the Directors, who act gratuitously, and are found to conduct the business entrusted to them with great assiduity and skill.

On the 11th of July there will be a small Eclipse of the Moon, *partly visible*, the times are as follows:

		D.	H. ′ ″
The Eclipse begins,	July 11.		3 17 35
Moon sets eclipsed,	-		3 41 40
Middle of the Eclipse,	-		4 1 46
Ecliptic opposition,	-		4 12 36
End of the Eclipse,	-		4 45 57

Digits eclipsed, 1° 38′ 11″, on the south part of the Moon's disc.

On the 6th of August there will be an Occultation of the *Georgian Planet* by the Moon. The following are the Elements, and principal results of the calculation:

	D. H. ′ ″
Geocentric ☌ of the ☽ and ♅, Edinburgh, *Mean Time*, August 6.	22 3 48,09
Apparent Time,	21 58 18,20
Geocentric conjunction in Longitude, - - -	282° 36 42,46
Sun's Right Ascension, - - -	136 45 43,89
—— horary motion in Right Ascension, - -	2 23,62
Obliquity of the Ecliptic, - - -	23 27 44,40
Moon's Latitude North increasing, - - -	18 24,19
Geocentric Latitude of ♅, South, - - -	25 7,43
Moon's horizontal parallax for the Latitude of Edinburgh, -	54 25,90
—— horizontal semidiameter, - - -	14 52,01
—— horary motion in Longitude, at the instant of conjunction,	30 4,86
—— —— —— for the hour preceding, -	30 5,26
—— —— —— for the hour following, -	30 4,47
—— horary motion in Latitude, at the instant of conjunction,	+ 2 44,67
—— —— —— for the hour preceding, -	+ 2 44,73
—— —— —— for the hour following, -	+ 2 44,61

The following are the principal results obtained in making the calculation for Greenwich, reduced Lat. 55° 47′ 32″, Long. in time 12′ 41″.4 West of Greenwich:

	For the Immersion.		For the Emersion.	
Apparent Civil Time,	H. ′ ″	H. ′ ″	H. ′ ″	H. ′ ″
Instants assumed,	21 46 18,20	21 47 18,20	22 52 18,20	22 53 18,20
Right Asc. of the meridian,	283° 19 47,7	283° 34 50,1	299° 52 25,6	300° 7 28,0
Moon's true Longitude,	282 30 41,47	282 31 11,57	283 3 46,15	283 4 16,22
—— true Latitude, N.	17 51,26	17 54,00	20 52,35	20 55,07
Altitude of Nonagesimal,	12 27 47,5	12 31 25,0	17 40 28,3	17 46 38,6
Long. of Nonagesimal,	306 54 25,9	307 30 7,5	337 [illegible] 19,2	337 32 15,1
Parallax in Longitude,	— 4 52,03	— 5 0,07	— 13 25,55	— 13 33,74
Parallax in Latitude,	53 15,52	53 14,78	51 56,96	51 55,16
Apparent diff. of Long. of the ☽ and ♅,	10 53,02	10 30,96	13 38,14	14 0,26
Apparent diff. of Lat. of the ☽ and ♅,	10 16,83	10 13,35	5 57,18	5 52,66
Moon's apparent motion in 1′ of time,		18,34		23,14
Errors from instants assumed, - -	— 0,74	+ 17,60	6,43	16,71
	Immersion, D. 6. H. 21 46′ 22″,66		Emersion, D. 6. H. 22 52′ 37″,87	

No allowance has been made for the semidiameter and horary motion of the Planet.

The final results in one view are as follows, *mean time*,

	D.	H. ′ ″		′ ″	
Immersion,	6.	21 51 52,56	at	10 16,8 N.	of the ☽'s centre.
Emersion,	-	22 58 7,[illegible]		5 55,9 N.	

At Aberdeen, the evening of the 5th of April was very favourable for observing the Occultation of Jupiter and his Satellites by the Moon, in as far as regarded the Immersions. The following are the times of these phenomena, after allowing for the error and rate of the clock, which were ascertained by transits of the Sun :

	By Observation.				By Calculation			
	D.	H.	′	″	D.	H.	′	″
Immersion III. Satellite, April	5.	23	6	52,7		—	—	—
——— I. ——— ———	–	23	10	5,2		—	—	—
First External Contact,	-	23	17	37,7	5.	23	17	[illegible]
First Internal Contact,	-	23	18	40,2		23	18	7,81
Immersion II. Satellite,	-	23	22	25,7		—	—	—
Second Internal Contact,	6.	0	[illegible]	12,5	6.	0	8	51,03
Second External Contact,	-	0	10	15,5		0	9	52,83

The Immersion of the IV. Satellite happened at so short an interval from that planet, that it could not be conveniently attended to. The times of the immersions are supposed to have been obtained to the nearest half second; but a haze which afterwards accompanied the Moon, rendered the Emersions of the Satellites invisible, and the two observations of the emersion of the planet subject each to an uncertainty of two or three seconds. The telescope used was a 3½ feet achromatic one by Dollond, with a power of about 70.

Art. XXIV.—*Proceedings of the Royal Society of Edinburgh.* (Continued from Vol. X. p. 353).

April 5. 1824.—" *On the Application of Professor Dœbereiner's late Discovery, to Eudiometry,*" by Dr Edward Turner. (This paper is printed in the present Number of this Journal). The same evening there was read a paper " *On the Quartz District in the vicinity of Inverness,*" by Mr George Anderson.

April 19.—There was read a memoir by Dr Brewster, " *On the Optical and Mechanical Structure of Minerals which form the Composite System,*" which it is proposed to add to other Systems of Crystallography.

May 3.—There was read " *Conclusion of Dr Turner's Memoir on Eudiometry.*" At this Meeting, the following gentlemen were elected members:

William Wood, Esq. Surgeon.
Dr W. C. Mair, Physician to the Embassy to Mexico.

May 17.—" *Account of two Surfaces of Siliceous Velvet, incapable of reflecting light, and produced by the fracture of a large Crystal of Quartz,*" by Dr Brewster.

Art. XXV.—*Proceedings of the Wernerian Natural History Society.* (Continued from Vol. X. p. 357).

March 20. 1824.—THE Secretary read a paper, by Mr John Murray, Lecturer on Chemistry, "*On the Ascent of some species of Spiders into the Air,*" shewing that this depends on their fine gossamer threads being peculiarly affected by the electric fluid of the atmosphere: Also two papers by the Reverend Dr Fleming of Flisk; the one containing an account of a new British species of Echinus; and the other a description of a new species of Plumularia. At the same meeting was read a communication from Prideaux John Selby, Esq. of Twizell House, containing some curious particulars in the natural history of the Golden-crested Regulus.

April 3.—Mr F. A. Parry read a memoir "*On the Management of Young Plantations of Forest-trees.*"

At the same meeting, Dr Robert Knox communicated some remarks on the supposed discoveries of Professor Tiedemann and Dr Fohmann, relative to the non-existence, in the Phoca vitulina, or common seal, of the *vasa efferentia.*

April 17.—Dr Knox read a short paper "*On the black colour of the periosteum in the Colymbus septentrionalis or Red-throated Diver.*"

Mr John Deuchar, Lecturer on Chemistry, communicated some remarks on Meteoric Stones, and proposed a theory to account for their formation.

At the same meeting, there was laid before the Society the first part of a very copious descriptive account of the Sand-field in the vicinity of Edinburgh, by Mr Blackadder; with illustrative sketches.

Art. XXVI.—SCIENTIFIC INTELLIGENCE.

ASTRONOMY.

1. *Planets Venus and Mars.*—The celebrated astronomer Schrœter maintains, that the mountains in Venus, are infinitely higher than those in the Earth. According to his observations, some of them must be seven times higher than Chimborazo. This statement is denied and disproved by Gruithuisen.

The same astronomer agrees with Schrœter, Herschel and others, in considering the white polar spots at the south pole of Mars, as owing to ice and snow; and the spots he observed in the polar regions of Venus he ascribes to the same cause. Let it not be objected that Venus is nearer the Sun than Mars. We are also nearer the Sun than Mars, and yet the Earth has a much larger spot of snow than that planet. We know, that the heat at the surface of a planet may not depend so much upon the proximity of the sun, as upon the density of the atmosphere.

2. *The Moon.*—Gruithuisen has published some curious observations on the physiognomy of the Moon's surface. He compares the lunar mountain ranges and groupes, with those of the Earth, and offers conjectures as to their geognostical structure, and composition. From the external characters of the ranges, he considers himself entitled to assume the existence of primitive rocks, of various secondary limestones, &c. The lunar atmosphere, according to his observations, from its nebulous strata, must contain water. Such an atmosphere is probably intended to assist in the support of organized beings, and our author maintains the existence of such in the moon. Schrœter, the great astronomer, often speaks of the arts, the civilisation and the industry of the inhabitants of the moon. He conjectures the existence of a great city to the north of *Marius*, (a spot in the moon), and of an extensive canal towards Hygena (another spot); and lastly he represents a part of the spot named *Mare Imbrium* to be as fertile as the Campania. Gruithuisen enters into a long discussion regarding the existence of water in the moon, and finishes by maintaining the existence of lunar seas, lakes, and rivers.

NATURAL PHILOSOPHY.

3. *Perkins' Steam-Engine.*—The *Bibliotheque Universelle* for March 1824, contains an elaborate paper on Mr Perkins' Steam-Engine, by a friend of Mr Perkins, which was carried to Geneva, and communicated to the editors, by Mr Church, the American Consul, who had made a voyage to London for the express purpose of seeing Mr Perkins's apparatus. This paper contains the most complete description of the above engine which has yet appeared, and it presents, we believe, the first

attempt to explain its operation on philosophical principles. We have been anxious, therefore, more particularly on the latter ground, to examine it, having hitherto looked in vain for any rational account of Mr Perkins's plans, or of those advantages arising from them which have been so confidently asserted as a matter of fact, but which we confess we have been from the beginning doubtful of, from what is already known of the nature and principles of heat and of steam. Having read the paper, however, we really see nothing advanced in it which tends in the least to alter the opinions we had previously formed; and those who expect in it any reasons to satisfy their curiosity or belief, will undoubtedly be disappointed. In place of that clear and philosophical exposition of causes and effects which such a subject demands, and certainly admits of, if any real discovery has been made, we are here presented with such a mass of mere theories and assumptions, together with such fanciful paradoxes, and downright absurdities, as we believe have seldom been brought forward in the shape of philosophy. Instead of proceeding with a plain statement of experiments, and of consequences deducible from them, or advancing clearly and boldly forward from principles already known, to some great and striking conclusion, the author is continually halting in his career, and bewilders himself in a maze of obscure and unintelligible speculation, ingeniously contrived, one would think, to puzzle himself and his readers. He appears to entertain, in some respects very correct views on the nature of heat, and its expansive force; but he has taken up some strange notion regarding its power of compressing a confined liquid, such as the water in the generator, and of forcing or squeezing out of it, "as from a sponge," the heat which it contains. This, and several other notions of a similar kind, seem to have confused his whole ideas of the subject he attempts to explain; so that, though his remarks on other points are, in many respects, sensible and judicious, yet on these topics he appears incapable of reasoning with his accustomed accuracy and vigour of judgment. We are often at a loss to know what he would be at; and all his endeavours to prove what he wishes to demonstrate, are vain. He occasionally proceeds so clearly and methodically with his principles, that you are prepared for some important consequences; instead of which you are landed in some ingenious paradox,—some palpable inconsistency,

—some result which turns out, after all, mere assertion or assumption, or not deducible at all from the premises; or, lastly, some obvious truth, in which you are surprised the author can discover any thing new or important.

Having only just received this paper, our limits do not permit us to enter more fully into the particulars of it in the present Number. We shall just state, therefore, in proof of what we have said, one or two, as a specimen of the propositions maintained there. 1. It is said, that, in the generator, or high-pressure boiler, the heat is greatest at the top, and decreases towards the bottom, against which the flame and heat of the furnace are chiefly directed; so that while the temperature of the upper parts of the boileris at 400°, that of the lower parts next the fire may, in extreme cases, be so low as 40°. 2. Although the water exposed in this manner to the intense heat of a furnace, remains permanently cold, yet, if any crack or opening should take place in the bottom of the boiler within which the water is pressed with a force of at least 400 lb. on the inch, yet no water will issue at the opening. The reason assigned for this, we are unable to comprehend, or to render intelligible.* 3. It is proposed to "pump back the heat" into the boiler, after it has done its office of impelling the piston in the cylinder; to pump it back into the generator, and to cause it in this way to act again and again upon the piston; so that, in this manner, the author, in the fervour of his imagination, thinks it but reasonable to expect, that an apparatus of this kind may be constructed, which, when once sufficiently heated, will continue to move for ever, and to drive machinery of itself, without any farther consumption of fuel. On looking into his description of this part of the apparatus, we find the plan consists merely in heating the water of the generator by the waste steam from the cylinder,—a plan which has been already frequently proposed, and which is indeed practised to a certain extent in every steam-engine in the kingdom.

HYDROGRAPHY.

4. *Marobia.*—The "Marobia" is an extraordinary phenomenon, most probably deriving its name from Mare Ubbriaco, or Drunken Sea, as its movement is apparently very inconsistent. It occurs principally on the southern coast of Sicily, and is generally found to happen in calm weather, but is considered as the

certain precursor of a gale. The marobia is felt with the greatest violence at Mazzara, perhaps from the contour of the coast. Its approach is announced by a stillness in the atmosphere, and a lurid sky; when suddenly the water rises nearly two feet above its usual level, and rushes into the creeks with amazing rapidity: but in a few minutes recedes again with equal velocity, disturbing the mud, tearing up the sea-weed, and occasioning noisome effluvia: during its continuance, the fish float quite helpless on the turbid surface, and are easily taken. These rapid changes, (as capricious in their nature as those of the Euripus), generally continue from thirty minutes to upwards of two hours; and are succeeded by a breeze from the southward, which quickly increases to heavy gusts.—*Smyth's Memoir of Sicily.*

5. *Mediterranean.*—The medium heat of the sea around Sicily, at a depth of from ten to twenty fathoms, by Six's thermometers, is from 73° to 76°, which being 10° or 12° warmer than the water outside Gibraltar, accounts for the greater evaporation and consequent currents.—*Smyth's Memoir*, p. 184.

MINERALOGY.

6. *Mountain Tallow.*—Specimens of this mineral substance were lately found in a bog on the borders of Loch Fyne. This curious mineral was first observed by some peasants on the coast of Finland in 1736; afterwards it was found in one of the Swedish lakes. M. Herman, physician at Strasburgh, observed a similar substance in the water of a fountain near that city; and Professor Jameson met with it in this country. It has the colour and feel of tallow, and is tasteless. The following notice in regard to it was sent to us:—It melts at 118° and boils at 290°; when melted, it is transparent and colourless; on cooling, becomes opaque and white, though not so much so as at first. It is insoluble in water, but soluble in alcohol, oil of turpentine, olive oil, and naphtha, while these liquids are hot; but it is precipitated again when they cool. Its specific gravity in the natural state of it, is 0.6078; but the tallow is full of air-bubbles, and, after fusion, which disengages the air, the specific gravity is 0.983, which is rather higher than that of tallow. It does not combine with alkalies, nor form soap. Thus it differs from every class of bodies known:—from the fixed oils, in not forming soap;—from the volatile oils and bitumens, in being

tasteless and destitute of smell. Its volatility and combustibility are equal to those of any volatile oil or naphtha.

7. *Natural Carbonate of Soda.*—M. Rivero of Santa Fe de Bogota, informs us, that he finds the following to be the constituent parts of the Natural Carbonate of Soda, of the Lake of Merida in Columbia: 0.3900 carbonic acid, 0.4122 soda, 0.1880 water, 0.0098 loss.

8. *Rare Minerals found in the vicinity of Edinburgh.*—Within these few months, beautiful specimens of deeply coloured Prehnite have been found in Salisbury Craigs. In several of these, the prehnite was associated with that kind of *Datolite* described under the name *Humboldite* by Mr Levy. In the basaltic rock of the Castle Hill, small, but beautiful specimens, of radiated *Wollastonite* have been found. Many years ago, fine specimens were got. The same mineral occurs occasionally in the greenstone of Salisbury Craigs.

GEOGNOSY.

9. *Cave lately discovered near Killin.*—A cave was lately discovered in a rocky hill near to this place, by following a fox which had entered it on being pursued. It is said to be contained in a precipice of primitive limestone. The opening is small, but the interior is asserted to contain chambers in number equal to those of the inn. It has no appearance of being an artificial excavation, and is said to be distressingly cold, and very dark. The existence of such a cave had been traditionally talked of, as having been the resort of a famous freebooter, who was betrayed by a paramour, to whom Duncan Dhu offered as a reward as much gold as she could contain in her two hands. The gold, however, having been delivered in a melted state, the recompence proved perfectly suitable to the service.

10. *Ferussac's Edition of the Epoques de la Nature.*—Ferussac is preparing an edition of Buffon's *Epoques de la Nature,*—a work which will still be read with interest, as it contains many good ideas along with others fantastical and wild. The same active naturalist proposes to publish a description of the fluviatile and marine shells of the tertiary rocks of different parts of Europe, accompanied with geognostic descriptions, but will not, we believe, adopt the opinions of Brongniart, in regard to fossil organic remains.

11. *Prevost's Journey to England.*—Prevost, a good zoologist and excellent geognostical observer, visits England this summer, in order to study the transition and secondary formations; then goes to Normandy, with the view of comparing the rocks of that country with those of England; and the results of this investigation will be published at Paris next winter. Desnoyers, an active and skilful observer, will join Prevost in Normandy.

12. *Supposed Salt Formation above Shell-Limestone.*—In Karsten's Archives, there is a paper by Oeynhausen, on salt deposites, in which he endeavours to shew, according to Hausman, that there is a deposite of variegated and gypseous marls, analogous to the variegated sandstone above the shell-limestone. This, however, we are informed by Dr Boué, is an error; for these marls belong to the *Quader Sandstone.*

13. *Boué's Arrangement of Tertiary Rocks:*

First Arenaceous deposite.	Plastic Clay and Lignite.
First Tertiary Limestone.	Calcaire grossier, and Lignite.
Second Arenaceous deposite.	Gypsum, Marl, Sand.
Second Tertiary Limestone.	Limestone.
Fresh-water Limestone.	Calcaire d'eau douce.

CHEMISTRY.

14. *Discovery of Selenium in the Volcanic Rocks of Lipari.*—Dr Turner has just sent to us the following interesting notice, the substance of which was communicated to him by Professor Stromeyer.—"Professor Stromeyer has lately discovered Selenium under two different forms, one of which is altogether new. On diluting some fuming sulphuric acid, such as is made at Nordhausen from the sulphate of iron, he observed that a solid matter separated from the diluted acid, which, on examination, proved to be selenium. One pound of the acid gave, on dilution, about a grain of selenium. This substance has already been detected in some of the Bohemian sulphuric acid; and it is supposed that the acid in question had been prepared in Bohemia. The second source of selenium is in the volcanic productions of the Lipari Isles, among which Professor Stromeyer has lately discovered a native sulphuret of selenium. He has mentioned neither the mineralogical character of the new mineral, nor given an account of his chemical ex-

amination of it; but I hope soon to obtain from the Professor more full information on the subject, and shall then have great pleasure in communicating it to you. It appears probable, from these circumstances, that selenium is a more common production of nature than is generally supposed; and it may be anticipated, that it will frequently be found, whenever the attention of chemists and mineralogists in general shall be directed to the subject."

15. *Iodine and Phosphorus.*—" Thenard asserts, that, in the union of iodine and phosphorus, not only caloric, but light, is extricated. But Sir H. Davy states that no light is evolved in this process. Repeated experiments have convinced me of the accuracy of the observation of the British chemist; but it is only justice to M. Thenard to state, that, in the action between these substances, the evolution of light, as well as of caloric, may be shewn, according to the mode of making the experiment. If a small piece of dry phosphorus be dropt into a test-tube, and a quantity of iodine, in its usual scaly form, sufficient to *cover* the phosphorus, be quickly added, an immediate action ensues; the tube becomes hot; fumes of iodine are disengaged; and a deep violet-brown liquid is formed, *without the evolution of light*, even when the experiment is made in a darkened room. But if the proportion of the phosphorus to the iodine be large, and the latter insufficient to cover the former, the action is accompanied by a momentary flash, which I attribute to the combustion of the uncovered portion of the phosphorus in the scanty portion of atmospheric air in the tube. By varying the proportions of the two substances, I can produce the union with or without the extrication of light at pleasure."—*Letter from Dr Traill to Professor Jameson.*

ANTHROPOLOGY.

16. *Races of Men.*—" The physiognomy of the Chinese colonists was particularly interesting to us, and was, in the sequel, still more so, because, we thought, we could perceive in them the fundamental lines which are remarked in the Indians. The figure of the Chinese is, indeed, rather more slender, the forehead broader, the lips thinner, and more alike, and the features, in general, more delicate and mild than those of the American, who lives in the woods; yet the small, not oblong, but roundish,

angular rather pointed head, the broad crown, the prominent sinus frontales, the low forehead, the pointed and projecting cheek-bones, the oblique position of the small narrow eyes, the blunt, proportionally small, broad, flat nose, the thinness of the hair on the chin, and the other parts of the body, the long smooth black hair of the head, the yellowish or bright reddish tint of the skin, are all characteristics common to the physiognomy of both races. The mistrustful, cunning, and, as it is said, often thievish character, and the expression of a mean way of thinking, and mechanical disposition, appear, in both, in the same manner. In comparing the Mongol physiognomy with the American, the observer has opportunity enough to find traces of the series of developments through which the Eastern Asiatics had to pass, under the influence of the climate, in order, at length, to be transformed into an American. In these anthropological investigations, we arrive at the remarkable result, that certain characteristics, which constitute the principal difference of the races, do not easily pass into others, whereas those which depend only upon *more* or *less*, gradually vanish or degenerate, through a series of different gradations. In this respect the difference of the Negroes is peculiarly striking, who, in various particulars, especially the complexion, the hair, the conformation of the skull, the proportions of the countenance, and of the whole body, differ more from all other races than from each other. The Negro races of the South Sea, and the Indian Archipelago, who, for the most part, are derived from a mixture of various races, who, at so great a distance from their native country, must experience considerable modification of the Ethiopic character, yet still indicate, in every respect, their African descent, rather than a nearer affinity with the other races. On the other hand, the physiognomical characteristics of the Mongol, Caucasian, Malay, and American races, blend with each other through so many shades, that we are involuntarily led to presume a common fundamental type for all these, in opposition to the Ethiopic, which perhaps is most strikingly marked in the Mongol, and to which the above mentioned various conformations must perhaps be referred to so many forms of development, occasioned by climate, as has been already asserted, by a very distinguished writer on Universal History.

Whether such a change, proceeding from the aboriginal inhabitants of Upper Asia, has really produced the actually existing four chief varieties of the Mongol as the oldest, then the American, the Malay, and Caucasian, would form one of the most important and interesting investigations for the study of anthropology, as well as the history of the revolutions of the earth in general."—*Spix and Martin's Travels in Brazil.*

17. *Colour of the Negro.*—In a former number of this Journal, we mentioned the opinion of Professor Rudolphi of Berlin, in regard to the seat of the black colour in the Negro race. That distinguished anatomist infers from the dark colour of the epidermis, that it is the principal seat of the colour. On conversing lately with an intelligent American anatomist, who has dissected, in his theatre, many Negroes, I was informed that he found the epidermis to be nearly black, and the rete mucosum, the supposed seat of the black colour according to many anatomists, had nearly the same tint of colour as in the European race.

ZOOLOGY.

18. *Seal and Walrus.*—Sir E. Home, in a paper read before the Royal Society of London, mentions several facts he has discovered in regard to the walrus and seal.—The first discovery was, that the walrus is provided with means similar to those of the fly, which enable it to walk in an inverted position. The structure of the foot of the fly is described in the Philosophical Transactions for 1816; and on seeing a mutilated foot of the walrus, Sir Everard was struck with their mutual resemblance, and requested Captain Sabine, who was about to visit the Arctic regions, to bring him a specimen, which, with the aid of the assistant-surgeon of the ship, he accomplished. On examination, the hind-foot was found provided with a hollow space, which enabled the animal to produce a vacuum when it was placed on any surface; the foot was provided with two toes, which enables the creature to form a more perfect vacuum, by bringing the parts in closer contact with the surface on which the foot is placed, and which also admit the air on their being raised. In describing the foot of the fly, similar points or toes we observed, but it was not until an opportunity occurred of re-

examining the same organs in a larger creature, that their use was fully developed. The foot of the walrus required diminishing four times to exhibit it in a quarto plate, whereas that of the fly required magnifying 100 times to render it distinctly visible. —The second fact relates to the internal formation of the same animal. A reservoir of a cylindrical form, the coats of which are covered with a thick mucus, receives the bile by a lateral communication, and it then propels it into the duodenum. From the width of the œsophagus large masses of food can be swallowed; the animal possesses the power of regurgitation; the opening of the pylorus is comparatively small, and is valvular, by which the contents of the stomach are prevented from passing into the duodenum. These parts, and many of the external organs, are of a similar structure to that of the seal. Mr Forster states the food of the walrus to be the *Fucus digitatus*, which is found on the Arctic shores, and also under the ice.—The third subject of remark was the funis and placenta of the seal. The vessels of which the funis is composed, are about nine inches long, and are not twisted; and they anastomose at about three inches distance from the placenta, which is connected with them by three membranous coats: this conformation admits of great freedom in the fetal circulation.

19. *Notice in regard to a Fossil Whale discovered in Dunmore Park.*—Mr Blackadder, in a memoir lately read before the Wernerian Society, announced the discovery in Dunmore Park, in Stirlingshire, of the remains of a whale imbedded in alluvial soil. The Earl of Dunmore, in whose grounds it occurs, in a letter to us on this subject, says, "I find that its whole remains lie from about half a mile to three quarters of a mile from the present bed of the river. It is covered by three or four feet of alluvial soil, and is rather more than 20 feet above the ordinary spring-tides. Its position above the level of the tide-water is about the same as that of the whale found at Airthrie. Its length is about 70 or 75 feet. At present it supports a good crop of wheat; but after that is removed, it shall be carefully disinterred for the College Museum."

20. *Habits of the Whale.*—In October last, a whale appeared in the river St Lawrence, as high up as the city of Montreal. The animal was pursued by numbers of boats, and was at last

taken at Boucherville, a distance of 9 miles from town. It was exhibited at Montreal, then towed down for exhibition to Quebec. This animal must have come from the Whale Bank off Newfoundland, which is the nearest place where whales are generally found, and have wandered at least 1000 miles in a straight line up the gulf and river before it was taken, 350 or 400 miles of which must have been fresh water.

COMPARATIVE ANATOMY.

21. *Structure of Electric Organs of Gymnotus electricus.*—On each side there is an upper large and an under small electric organ. The large organ arises immediately behind the head, under the great dorsal muscles, where it is obtuse, and gradually becomes narrower, and terminates acutely towards the end of the tail. It is straight, or somewhat hollowed, towards the back-bone, but convex in the opposite direction: above, it terminates in a line; in a nearly similar manner below, and is thickest in the middle. It is composed of horizontal plates, about the third of a line from each other, which are traversed at right angles by partition walls, and in the small space between them water is contained. Below this large organ lies a similar, but smaller and more minutely divided one. These parts are supplied with numerous intercostal nerves: in the specimen examined by Rudolphi, 224 were observed on each side. A great branch of the third branch of the fifth pair of nerves, augmented by a smaller branch from the vagus, runs parallel with the back-bone, from the head to the tail, where it divides. This nerve runs immediately over the intercostal nerves, and crosses them at right angles, without, however, uniting with them in any way; on the contrary, they are entirely distributed to the muscles of the back. This is the nerve which Hunter describes as the vagus, and which Fahlberg erroneously considered as the electric nerves; while Hunter, with his usual accuracy, describes the intercostal nerves as those of the electric organs. If we compare the electric organs of the torpedo and gymnotus electricus, the first may be compared with the voltaic pile, the second with the trough apparatus. But they agree in their principal feature, viz. in the abundant distribution of nerves to very vascular plates, between which a serous fluid is disposed.

BOTANY.

22. *Luminous Plants.*—It is well known that some plants are luminous; and also that parts of plants in an incipient state of decomposition, shine more or less. The following instances of this property are given at present; it being our intention, in a future number of the Journal, to enter fully on this subject. 1. Potatoes, kept in cellars, in a growing state, and therefore useless as food, sometimes become so luminous, that we can read by them the print of a book in the dark. 2. The Dictamnus albus spreads around it, in dry summer evenings, an atmosphere which, on the approach of a taper, inflames with a bright blue flame. 3. Other plants give out a *sparkling light*, probably of an electrical nature; such is the case with the flowers of Calendula, Tropæolum, Lilium bulbiferum and chalcedonicum, Tagetes, Helianthus, and Polyanthes, as mentioned by Mr Johnson of Wetherby in Vol. iii. p. 415. of this Journal. 4. Some plants give out a calm steady light, of a bluish, greenish, or yellowish-white colour, such as Dematium violaceum, *Pers.*; Schistostega osmundacea, *W.* and *M.*; Phytolacca decandra, Rhizomorpha pinnata, *Humb.*, &c. The luminous appearances in the galleries and shafts of our mines are often to be traced to rhizomorphous plants. 5. The milky juice of some plants is very luminous. 6. Trunks, branches, and roots of trees, in an incipient state of decomposition, become luminous.

ARTS.

23. *Extraordinary extent of the Baize and Flannel Manufacture at Rochdale.*—"In the town of Rochdale, and the adjacent villages, there are manufactured, every week, of flannels and baizes, about 20,000 pieces, of 46 yards each, making 47,840,000 yards per annum. It is supposed that 17,840,000 yards are exported; the remaining 30 millions of yards are consumed in the United Kingdom, being an average of about 1¼ yards for each individual. Some good flannels are manufactured in Wales; a few coarse ones at Keswick, and some other towns and villages in the kingdom. A few are manufactured on the Continent, and works for that purpose are now erecting in America; but the whole of the flannels manufactured on the globe, besides those manufactured in Rochdale and its immediate vicinity, are not equal in quantity to those made there.

The price of flannels is from 5d. to 3s. per yard; and the average may be stated at from 13d. to 14d. per yard; so that the annual value of the manufacture may be stated at about three millions Sterling. The wool costs fully one-half of the whole-sale selling price; the oil, labour, and finishing, &c. constitute nearly the other half."

OBITUARY.

24. *Biographical notice of Otto Fabricius.*—The sciences have lost in the course of last year two Danish naturalists of great reputation, Fabricius and Viborg, an account of whose lives is inserted in the 6th number of the *Journal of Natural History* of Messrs Oersted, Horneman and Reinhards, Copenhagen, 1823. We shall give an extract relative to the first-mentioned naturalist.—*Otto Fabricius* was born on the 6th March 1744, at Rudkiobing in the island of Langeland, of which his father was pastor. During his attendance at the university of Copenhagen, the reading of the works of Hans Egedé on Greenland, excited in him a desire to go to preach the Gospel in that country. He was ordained in 1768, and nominated missionary for the Danish colony of *Frederickshaab*, where he resided from five to six years, often living in the huts of the Greenlanders, even accompanying them on their expeditions for the killing of seals, and neglecting no opportunity of acquiring a perfect knowledge of their language, and of observing the productions of the country. His application made amends for all the privations connected with this sort of life. Without any preliminary instruction, and without any other book than the *Systema Naturæ*, he became a naturalist by his own exertions, and the correspondence of the celebrated Otto Frederick Müller, who furnished him with advice. On returning to Copenhagen in 1773, he was successively appointed to several cures, both in Norway and Denmark. From 1789 to the time of his death, he occupied that of Christianshavn, to which the king attached for his sake the title and rank of bishop. He died at the age of seventy-nine years, on the 20th April 1822. He was twice married, and was the father of sixteen children. His principal work on Natural History, and that which has particularly made him known to foreigners, is that which he published at Copenhagen in 1780, under the title of *Fauna Grœnlandica.*

ART. XXVII.—*List of Patents granted in Scotland from 10th March to 19th May 1824.*

42. TO JOEL SPILLER of Chelsea, county of Middlesex, engineer, for "an improvement or improvements in the machinery to be employed in the working of pumps." Sealed at Edinburgh 18th March 1824.

43. To JEAN JACQUES SAINTMORE of Belmont Distillery, Wordsworth Road, Vauxhall, parish of St Mary, Lambeth, county of Surrey, distiller, for "improvements in the process of, and apparatus for distilling." Sealed at Edinburgh 1st April 1824.

44. To DANIEL TONGE of Liverpool, County Palatine of Lancaster, ship-owner, for "an apparatus, by means of which an improved method of reefing sails is effected." Sealed at Edinburgh 29th April 1824.

45. To BENJAMIN ROTCH of Furnival's Inn, city of London, Esq., for "an improved fidd for the upper masts of ships and other vessels." Sealed at Edinburgh 29th April 1824.

46. To THOMAS GETHEN, formerly of Weston Court, county of Hereford, now of Henry Street, Pentonville, Middlesex, gentleman, for "improvements in the machinery and process of making metallic plates, rollers, pipes, cylinders, and certain other articles." Sealed at Edinburgh 29th April 1824.

47. To JOHN GIBSON, woollen-draper and hat-manufacturer in Glasgow, for "the manufacturing or making of an elastic fabric from whalebone, and the manufacturing or making of elastic fabrics from whalebone, hemp, and other materials combined, suitable for making into elastic frames or bodies for hats, caps and bonnets, and for other purposes; and also the manufacturing or making of such elastic frames or bodies from the same materials, by the mode of plaiting." Sealed at Edinburgh 19th May 1824.

48. To WILLIAM YETTS of Great Yarmouth, county of Norfolk, merchant and ship-owner, for "certain apparatus to be applied to a windlass." Sealed at Edinburgh 19th May 1824.

P. NEILL, Printer.

THE

EDINBURGH

PHILOSOPHICAL JOURNAL.

ART. I.—*Observations on the Results of the late Expedition of Captain* PARRY, *including a View of previous Discoveries made in the same direction*. By HUGH MURRAY, Esq. F. R. S. E. Author of "the History of Discoveries in Africa and Asia," &c.

EVER after the grand era in the history of Navigation, and of the World, formed by the discovery of a Western Continent, the boundaries of this continent, and its junction with Asia, formed objects, that called forth and baffled the mightiest efforts of maritime enterprise. These features, hid in the remotest depth of both continents, and amid regions subject to the sway of perpetual winter, could not be approached unless by the boldest navigators; and even they, in general, soon thought themselves too happy in effecting their escape. No sphere of exertion has made a grander display of the prowess and daring of British seamen; for, it is with pride we reflect, that this career has been almost exclusively theirs. Britain began, Britain carried on, and Britain, we trust, will complete the delineation of this last unknown boundary of the habitable earth.

A shorter and more direct route to the East Indies was the object of the voyage of Columbus; and, even after the disco-

very of vast unknown regions, in the West, it still appeared quite probable that there might be openings admitting of such a passage. After, however, Vesputius, Ojeda, and other adventurers, had beat in vain round the Gulf of Mexico, and had traced a vast mass of continent stretching southward, men's views were, with some reluctance, turned to the north, as to the only remaining quarter of hope. The first great exertion in this direction appears to have been made by Sebastian Cabot, a Venetian, in the service of England, alternately encouraged and neglected, but at length created General Pilot of England. Under the enlightened reign of Edward VI., Cabot and his son made one or more voyages along the coast of North America, traced the boundary of what is now the United States, and partly that of Newfoundland; but finding that the invariable and always increasing direction of the coast was north-east, the most unpromising possible, and that they were coming into the region of floating ice, and inclement skies, they were satisfied with the discoveries already made, and returned to England.

Cortereal, a Portuguese, sailed along the coast of Labrador, which, in the early maps, is called from him Corterealis, and probably he even traced the entrance of Hudson's Bay. It is certain that he returned with sanguine hopes of discovering a passage. for he soon after set out on a new expedition; but the issue was fatal; he returned no more His brother, Miguel, who went in search of him, shared the same fate. A third, who desired to follow, was stopped by the express orders of the King. With this enterprising and unfortunate family, seems to have expired the zeal of the Portuguese for discovery in a northern direction.

Spain, which claimed America as her own, might have been expected to take the lead in tracing its boundaries, and its relations with the neighbouring continent. Before, however, that power had succeeded in establishing full sway over her extensive dominions in the New World, her administration had begun to sink into that gloomy apathy in which it has ever since been involved. From the western coast of Mexico, however, she sent several expeditions, to discover the *Strait of Anian.* This celebrated name, which glittered for several centuries before the eyes of modern adventurers, has involved geographers

in much perplexity, and been considered even as wholly inexplicable. We are convinced, that it originated in a very different quarter of the world from that to which navigators have been accustomed to refer it. In the earliest maps, Anian is delineated as the most eastern country of Asia, and it occupies the position of Cachmehina, which has always, with the natives, borne the title of Anam. To understand how the separating Strait of Asia and America could be placed here, we must attend to the train of ideas which prevailed at that infant era of geographical knowledge. The American islands, when first discovered, were still viewed as part of the East Indies, in search of which the voyage of Columbus was undertaken. Even after they were proved to belong to a great mass of continent, that continent was supposed to be attached to, and to form the eastern boundary of Asia. The influence of those ideas may be clearly traced in a series of very curious maps, delineated in the commencement, and even in the middle of the sixteenth century, at Venice, then the centre of nautical information; and which are preserved in the King's Library. In the earliest of these maps, Asia and America are joined throughout their whole mass; and in one of them Cathay is even placed on the Gulf of Mexico. Under the influence of these impressions, it is easy to conceive how the early East Indian navigators, on coming to Anam or Cochinchina, where the coast first decidedly changes from east and west to south and north, might imagine that they were now between the two continents, and would soon come to the division between them, which might be named by anticipation the Strait of Anian. In one of the maps above mentioned, this Strait is represented as running up across the whole breadth of the two continents. The progress of navigation proved that no such Strait existed here; but still the idea was rooted in the mind of geographers, and they transferred it farther and farther north, till they reached the frozen extremities of Asia. It is not probable, however, that this impression was founded even upon the most remote tradition of Behring's Straits. The derivation above given, seems confirmed by the circumstance, that the idea of the Strait of Anian was always combined, not with that of a bleak and wintry passage, but of a smiling and fertile region, and even of gold; which

last association wonderfully heightened its empire over the imagination of mankind.

What efforts the Spanish Government might make from Mexico to explore the north-west coast, is concealed at once by that mysterious obscurity which it studiously throws over all its proceedings, and by the apathy with which such enterprises soon came to be viewed. The most remarkable voyage is that professed to have been made by Juan de Fuca, by birth a Greek, and who ultimately lived neglected in his native country. The Spaniards deny all knowledge of him or his voyage; but one Douglas, an Englishman, who met him accidentally at Venice, took down his narrative, which was to the following purport: That, after passing the 48th degree of latitude, he had entered the Strait of Anian, and having sailed for twenty days through its long and winding channel, and seen people on the shore covered with the skins of beasts, he had emerged into the North Sea, when, conceiving himself to have accomplish the object of his voyage, he returned. This narrative was accounted a fable, till Meares and Vancouver, in tracing the northwest coast of America, discovered Vancouver's Island, separated from the continent by a long and narrow channel, precisely similar to that through which Fuca described himself to have passed. The aspect of the country and natives, and the passage with the open sea, precisely corresponded. It became evident, therefore, that the old captain had merely committed an error of judgment when, in sailing through this channel, he supposed himself to be sailing between Asia and America, and the sea into which he emerged to be the Northern Ocean.

This seems the time to notice the narrative of Maldonado, which Mr Barrow, though he has presented us with a translation of the original MS. preserved in the archieves of Spain, hesitates not to pronounce a palpable and decided forgery. We confess that this does not appear to us quite so clear. Maldonado is admitted to have been a navigator of great eminence; and it is not very likely that the Spanish Government would have treasured up his narrative so carefully, if they had not had some reason to suppose it genuine. Maldonado reports, that, after passing through Davis's Straits, and sailing a great distance, first north-west, and then south-west, he came to the

Strait of Anian, through which he passed into the South Sea. It may be very safely pronounced that he never did any such thing. But we must observe, that he pretends not to have sailed more than a very small space through, or to have reached any ascertained point of this imagined sea. All his statements amount to this, that, after a long voyage, he passed through *a Strait* into *a Sea.* All the rest is mere inference. This strait, he says, *appears* to be what geographers name the Strait of Anian; and, "*if it be so,* it *must be* a strait having Asia on one side, and America on the other." In sailing along the opposite coasts, he says of one, "*we concluded* that all that coast belonged to America;" and of the other, "This country, according to the charts, *must belong* to Tartary or Cataia; and, at a few leagues from the coast *must be* situated the famed city of Cambala." All is inference, except the mere passage through a strait into a sea. Now, there appears nothing to us very strange, in supposing, that, after beating in perhaps a devious course round Baffin's Bay, with which his directions and latitudes seem very closely to agree, he may have entered one of the straits leading into Hudson's Bay, and, on seeing that wide expanse opening before him, may have imagined that it was the South Sea. Navigators, at that time, had no idea of the breadth of America, but imagined, that whenever they turned its northern point they would enter the Pacific; and this delineation is retained by Sanson, in the middle of the 17th century. Returning, as already observed, almost immediately by the route he came, he could have no opportunity of discovering his error. In supposing the allusion to the discoveries of Quiros fatal to the credit of the narrative, Mr Barrow does not advert, that, though the date assigned to the voyage be 1588, the account does not profess to be written till 1609, two years after these discoveries were known. (See Voyages to the Arctic Regions, App. p. 26.) The only thing which bears the marks of decided fable, is a strange story of their meeting on the south side of the Strait with an European ship coming from China to pass through it. It seems scarcely possible, however, not to consider this as a clumsy interpolation by another hand. It is entirely at variance with the rest of the document, the whole train of reasoning in which implies Maldonado as the first dis-

coverer, and is utterly foreign to any idea of this being already a great commercial thoroughfare.

In coming to the annals of solid and well authenticated discovery, we at once find Britain taking the lead. Even during the reign of Henry VIII., two expeditions were fitted out, in 1529 and 1536; but they did not reach beyond the coast of Labrador. The age of Elizabeth was that in which the maritime glory of Britain first shone unrivalled. Under her direction, Martin Frobisher, one of the earliest of the series of our great naval men, was sent three different voyages to search for a western passage. Frobisher beat through the sounds and inlets of Hudson's Bay, but unfortunately fell in with a species of black stone, which, by a certain process, afforded some particles of what was imagined to be gold. This black stone became instantly the primary object; and he did not consider himself as having at all failed, when he brought home his ships loaded with this ideal treasure. As its futility, however, soon became manifest, a general feeling of disappointment attached itself to the voyage, of which it had been the sole result; and for some time no further attempt was made.

In 1583, Sir Humphry Gilbert, a gentleman of good lineage, and of high spirit and enterprise, undertook a voyage for the discovery of the passage. It was most unfortunate; the principal vessel was wrecked off Sable Island, and most of the crew perished. Sir Humphry himself, attempting to return to Europe in a little bark, was wrecked near the Azores.

The nation was not discouraged. In 1583, Davis commenced his three voyages, which made an important addition to our knowledge of these northern tracts. Having discovered and passed through the strait which bears his name, he traced the coast of Baffin's Bay as far as Sanderson's Hope. On the west he looked twice into the broad entrance of Cumberland Strait; but his imperfect means, and the desertion of his comrades, prevented him from penetrating through it. Still, on his return from his third voyage, he reported the northwest passage as a thing in his apprehension quite probable. Notwithstanding this statement, repeated disappointment again produced a period of despondence and suspended enterprise.

In the beginning of the seventeenth century, George Weymouth (1602), and John Knight (1606), were fitted out by the Muscovy and Turkey Companies; and about the same time James Hall was four times sent out by the King of Denmark; but all these worthies returned without accomplishing any thing that merits record. The glory of making the next step in northwest discovery was reserved for Henry Hudson. His first voyages were directed to Spitzbergen and Nova Zembla. In the fourth, however, made in 1610, he passedthrough the straits, and entered the bay, or rather great sea, which have both continued ever since to bear his name. The issue of the voyage was deeply tragical. After a winter passed in the south-eastern parts of the bay, his men mutinied, put him ashore, and left this great navigator to perish on that inhospitable shore. Most of themselves, however, were overtaken in their voyage home, by a similar fate; and the tidings were brought to England by Abaccuc Pricket, Bylot, and others, who represented themselves as having in vain resisted the measures of the rest, yet have not escaped the suspicion of being partners in the crime.

Notwithstanding the sad catastrophe of this voyage, the discovery made by it was too splendid and promising not to rouse immediate attention. In the following year (1612), Sir Thomas Button was sent to follow it up. Sir Thomas having passed through Hudson's Straits, steered directly westward. Having passed through several hundred miles of open sea, he imagined himself already approaching, or even *in* the Pacific, when suddenly there appeared to the west a continuous land, stretching on every side as far as the eye could reach. In the dismay which this discovery occasioned, he gave to the place the appellation of *Hope checked*. He then sailed northwards, but, in consequence of misty weather and imperfect observation, did not discover the great opening of the Welcome.

Bylot and Baffin, in 1615, entered the bay, and explored a great part of the eastern shore of Southampton Island, but were stopped by the ice. Baffin, in a more fortunate voyage, traced the great bay which bears his name, after which the spirit of northern discovery fell asleep till 1631, when it revived with extraordinary force. Luke Fox, a hardy and enterprising naviga-

tor, to which character he sought with less success to add that of a wit and a *bel esprit*, was sent out under the auspices of Government; and, in the same year, the merchants of Bristol fitted out a vessel under Captain James. Fox traced the two great channels leading northwards out of Hudson's Bay; of which the most westerly received first through him the name of Sir Thomas Roe's Welcome; and he made the important observation, that a tide came down from the north, contrary to that general tide which came in by the straits, and prevailed through the greater part of the bay. He traced also a considerable part of the channel on the eastern side of the great island of Southampton, which Captain Parry, of whose discoveries it was also the theatre, has called the Fox Channel. Fox sailed along a considerable part of its eastern coast; but when he found this like to fail him, he began, without due reason, to think "he had made a scurvie voyage of it," and calling the farthest point of land reached "Foxe's Farthest," returned to England.

Captain James was overtaken by winter in the southern part of the bay, where, being unprepared to encounter the rigor of the season, he endured a series of sufferings from cold, the doleful narrative of which tended much to chill the ardour of future adventurers.

Although all attempts at a passage had failed, the information of adventurers shewed, that the regions bordering on Hudson's Bay contained an ample store of commodities to which the highest value was attached in Europe, particularly of furs. Some individual Englishman, with this view, settled on its coasts, and Groseille, an enterprising Frenchman, who had penetrated thither from Canada, laid before his Government the plan of a regular establishment. Neglected by them, he addressed himself to Prince Rupert, who then took the lead in every object tending to national improvement, and through whose influence Charles II. was induced to engage in the undertaking. Unfortunately, the country was still held in thrall to the system of exclusive companies; and that of Hudson's Bay was formed, under the strictest prohibition to any one else to traffic in any country which was or might be discovered within the Straits. The Company made an ample use of their monopoly, if, according to Forster, for English goods, which cost them L. 4000, they

obtained furs and other articles, which sold in England for L. 120,000, making a profit of L. 5250 per cent. This, however, does not include the expence of forts and shipping; and, considering the mismanagement to which all such companies are liable, we are ready enough to believe that their profits have been by no means of that enormous magnitude which is usually supposed.

The Company, by their charter, were taken bound to make the most strenuous exertions to discover the Strait of Anian and a western passage; notwithstanding which it is generally asserted, that their most indefatigable efforts were directed to the prevention of any such discovery. In 1719, however, the urgent voice of the public impelled them to send an expedition under Knight and Barlow, to discover the Strait of Anian. The issue was of the most disastrous nature: the ship, soon after entering the Welcome, was wrecked near Marble Island, and the whole crew perished. The natives told Hearne that the two last survivors went every day to the top of a neighbouring hill to spy if any aid was approaching, and returned with marks of the deepest disappointment. At length one of them died, and the other, in attempting to dig his grave, fell down above him, and expired.

This catastrophe, though no blame was ever imputed to the Hudson's Bay Company, served to favour their views of quenching the spirit of discovery. At length, in 1741, the zeal of Mr Dobbs, a gentleman of fortune and influence, succeeded in stimulating the Admiralty to fit out another expedition. Mr Dobbs's main hope rested on Captain Middleton, who had been long in the service of the Hudson's Bay Company, but who now proffered his exertions, and declared his conviction of the probability of finding a passage. Middleton was accordingly supplied with two vessels, and set sail in 1741; but, from the lateness of the season at which he arrived, was obliged to winter in the the bay. Next season, as soon as the season permitted, he began to sail up the Welcome, and his attention was attracted by a broad inlet, to which he gave the name of the Wager. Being detained in it by the state of the ice for several weeks, he conceived himself to have completely ascertained, that it was a mere bay, formed by a river. He could discover no tides unless what came from Hudson's Bay; and as

he ascended, the water appeared sensibly to freshen. As soon, therefore, as he could extricate himself from this position, he made his way northwards, till he came to a cape, which, from the direction in which it turned, had much the appearance of forming the north-eastern point of America. His crew were now full of joy and hope; but next day at noon, a sad reverse shewed that they were completely inclosed in a bay; to which, from this disappointment, Middleton gave the name of Repulse Bay. He now endeavoured to find an outlet on its eastern side, and having walked twelve or fifteen miles, and ascended a very high mountain, he obtained the full view of a strait eighteen or twenty leagues in length, and seven in breadth, extending south-east towards Cape Comfort, on the eastern coast of Southampton Island. The strait thus connected Repulse Bay with the main body of Hudson's Bay; and it appeared to Middleton that the strong tide which came through it, was merely that which entered by Hudson's Straits, and which, being brought by this circuitous channel into the Welcome, appeared there as a different and even contrary tide to that which came up, although both had the same origin. The strait was completely frozen from side to side, and, on calling a council, it was agreed "to make the best of their way out of this dangerous strait."

Middleton, on his return to England, reported the above observations, and proclaimed that every chance of discovering a passage here was completely at an end. Dobbs, greatly discontented, wrote several letters to him on this subject, and urged that there might be a passage farther north. Middleton answered, that "if there was, it must be impassable for the ice; and from the narrowness of any such outlet in 67° or 68° of latitude, it can be clear of ice only one week in the year, and many years, I apprehend, not clear at all." He would be happy to give any assistance in his power, but "hoped never to venture himself that way again." Although the communications had been thus far amicable, Dobbs describes himself as involved in many doubts; but his suspicions were blown into a flame by a singular letter received from two anonymous individuals. They told him that "all nature cried aloud there was a passage;" that this "script" was intended to open his eyes, and shew him "the

discoverer's pranks." They could not bear that so glorious an object should die through the mercenary baseness of its conductor. They were ready to venture their fortunes, their lives, their all, in another attempt; and they assured him, that "they were no inconsiderable persons." They proved in fact to be the surgeon and clerk of the ship; and Dobbs, on further communing with them, openly proclaimed Middleton as a traitor, bribed by the Hudson's Bay Company, who had offered him £5000, before he set out, to stifle the discovery. An embittered controversy followed. Middleton was strictly examined before the Admiralty, and wrote repeated defences of himself; but an unfavourable impression with regard to him remained both with Government and the public.

From the voyage next to be narrated, as well as that of Captain Parry, it appears fully ascertained, that Middleton was on this occasion most grossly injured. By these navigators all the features of the coast were found to have been most accurately described by him; and though his conclusions with regard to the origin of the tide in the Welcome, and the impossibility of penetrating farther, proved to have been hastily made, they were supported by very strong apparent presumptions. He appears only, after forming the opinion, to have betrayed rather an immoderate anxiety that no farther idea of any such attempt should be entertained. If indeed it was ascertained that he said, at the table of the Governor of Churchhill Fort, and in presence of some of his officers, that "he would make the voyage, and none on board should know whether there was a passage or not, and that he would be a better friend to the company than ever," there would be great reason to doubt his faith from the first; but it seems scarcely possible that he could have been so imprudent. The officers asserted, however, that when some of them doubted his conclusion of the Wager being a river, "he rated the clerk as a double-tongued rascal, would cane the lieutenant, broomstick the master, and lash any others" that should throw out such doubts. He is also said to have refused to bring home two Indians who were taken on board, or to hold any communication with them; though one of the officers, who had gained a smattering of their language, gleaned some notices from them respecting the Coppermine River,

and the sea to the north, and even procured a map, drawn on parchment with charcoal, shewing the way thither. Middleton also, on returning to England, appears to have repeatedly boasted, that, after such a navigator as he had failed, no one else would attempt to follow in the same tract.

These imprudent words and actions of Middleton, gave at least an additional colour to the charges against him; and Dodds conceived that "the demonstrations now of there being a passage, are as strong as they well can without actual passing it." A new expedition was therefore fitted out under Captains Moor and Smith. These navigators, after wintering in Hudson's Bay, were unable to proceed up the Welcome till July. They spent some weeks in exploring the Wager, which they completely ascertained to be a river. A council was then called, when it was decided that it was too late in the season to effect any thing farther, and that their best course was to return to England. Ellis infers from this resolution, "that there were some who began to be tired of so much labour and hardship, and who were therefore inclined to put an end to the voyage as soon as they could;" a conclusion in which we cordially agree with him. Notwithstanding this lame and impotent conclusion, it does not appear that the conductors of the expedition were visited with any of that storm of wrath which had rained thick as hail on the unfortunate Middleton.

The narratives of this voyage published both by Ellis and the clerk of the California, tended to encourage the idea of a passage; but after this repeated disappointment, the curiosity of the public again flagged. In 1775, however, a quite new light was thrown from a different quarter on the geography of America. Cook, the great circumnavigator, crowned his glories by discovering that long sought for strait which separated the two Continents. He even examined some part of the coast beyond; but the loss of this great man devolving the career on less zealous and devoted individuals, caused it to stop short. The observations of Cook, however, shewed the immense breadth of America at this latitude; whereas hitherto it had been usually supposed that its northern extremity terminated in a point, which being passed, the navigator would find himself in the South Sea, and in full sail for Japan and China. It

now appeared, that, after leaving the Atlantic, and before entering the Pacific, he would have above a fourth of the circumference of the globe of unknown sea to traverse. On seeing Hudson's and Baffin's Bays seemingly inclosed on all sides, and the coast from Behring's Straits stretching north, the impression arose, of a mass of land stretching indefinitively towards the pole, or at least not terminating short of the 80th parallel of latitude; and, upon this principle, all the maps were for some time constructed. A new and opposite light, however, rose from an opposite quarter. Hearne declares, that whatever reluctance against discovery the Hudson's Bay Company might have formerly shewn, all that he had seen had been of a quite opposite tenor. He himself was employed by them on an important land expedition to the north, partly in search of a copper mine reported to exist on a river there, and partly to ascertain whether, according to reports which came from various quarters, there was a sea in that direction. Hearne, being aided by a party of Indians proceeding with a most savage purpose against the Esquimaux, actually succeeded in reaching the river, the copper mine, and finally the shore of the northern sea. He placed it in about 73° North Lat.; but Mr Dalrymple, from Hearne's own data, cut it down to 69° or 70°. In 1789, additional information was obtained. At Montreal a body of merchants had formed themselves into a firm called the North West Company, for the purpose of carrying on the fur trade. With the activity of freedom, they had succeeded in supplanting, to a great extent, the favoured and privileged exertions of the Hudson's Bay Company. Their agent Mr Mackenzie, afterwards Sir Alexander, was employed to make several exploratory journeys, in one of which he discovered the sea in nearly the same latitude, and about twenty degrees to the westward of the mouth of Hearne's River. These facts suggested a view of American geography quite different from that which had been first suggested by Cook's discoveries. The sea being found at these two points, and at the Icy Cape of Cook, nearly in the same latitude, there arose the strongest presumption that, in or about that latitude, it washed and bounded the whole northern coast of America.

This new light thrown upon American geography, did not remain a matter of simple speculation. In an active and spirited naval administration, it revived the almost extinguished hope of a northwest passage. It appeared nearly certain that an ocean extended along the north coast of America, at a latitude quite consistent with the most free and safe navigation. If, therefore, from Hudson's or Baffin's Bays, there could be found an entrance into this ocean, the passage to Behring's Straits would probably be found unobstructed. Such was the principle upon which were founded the successive expeditions of Ross, Parry, and Franklin. We shall not now detain the reader, by even the most summary view, of results which must be so familiar to him. Suffice it to say, that Captain Parry found his progress from Baffin's Bay opposed by very serious obstacles, and was also led to the conclusion, that the closer he kept to the coast of America, the better chance he would have of working out a passage. In both these views, the attention of his employers was turned to Hudson's Bay. Middleton's facts had by no means disproved the possibility of finding an outlet somewhere or other; and their weight was diminished by the cloud which hung over his testimony. It was determined, then, to revive on this side the often repeated attempt. All the measures taken in former voyages for strengthening the ships, and storing them with ample supplies, were repeated, with such improvements as experience had suggested; and, in the spring of 1821, the new expedition, composed of the Fury, under Captain Parry, and, the Hecla, under Captain Lyon, was ready to start *.

On the 8th May, the expedition set sail from the Nore; but being detained by adverse winds, it was not till the 14th June that they reached Davis' Straits, and saw there the first iceberg. They were soon in Hudson's Straits, where mountains of ice

* We had prepared a somewhat full abstract of the events of this voyage; but considering that they have appeared before the public in so many shapes, and must be now familiar to all our readers, it appeared unnecessary thus to swell an article, which has already reached an unexpected length. It will be enough to exhibit a general outline of its results, as preparatory to considering the prospects of success in farther attempts at discovery in this direction.

were still tossing, and allowed only perilous passage. They considered themselves even fortunate in being inserted into a corner of one of these masses, and being for a week driven about along with it. August therefore was begun, before they reached that point on the eastern coast of Southampton Island, whence Baffin had turned back, and where their career of discovery was to begin. After great deliberation, they determined to attempt the much controverted Frozen Strait of Middleton; and, in fact, though some days were spent in exploring a wide bay, to which they gave the name of the Duke of York, this strait was found to exist, and to bring them into Repulse Bay. It too soon proved to be completely landlocked, and the whole coast to correspond with the description of Middleton. More than a month, however, had now been lost, in going over again his ground; and it was now their task to explore the coast beyond, leaving not an opening untried, by which it was possible that an entrance into the Polar Sea could exist. The coast, however, held now the unpromising direction of from west to east; and it presented a complete chaos of straits, bays, islands, and passages, blocked up with ice of every form and dimension. After exploring therefore a number of inlets, to the principal of which was given the name of Captain Lyon, the thickening symptoms of polar winter obliged them to saw a passage into the heart of a field of ice attached to an island, called by them Winter Island, and to lay themselves up for the season.

The tedious hours of this long winter were beguiled by dramatic entertainments, musical parties, and particularly by intercourse with a tribe of Esquimaux, who came to settle in their vicinity. From the conversation, and even the rude delineations of the latter, important notices were derived respecting the shores beyond. It was stated that the coast, which had hitherto run eastward, would now take a northerly direction; that it belonged to a great peninsula, forming the north-east extremity of America, and on the western side of which was a vast extent of ocean. The peninsula was bounded on the north by a strait leading into that ocean; and on the other side was a large tract of insular land, called Keiyuk Tarruoke; north of which, again, was another strait, similarly opening into the western sea. All this proved substantially correct. Having employed the best

half of June in sawing through 2000 feet of environing ice, they set sail on the 3d July, and about the middle of the month saw before them the great northern insular tract, which they chose to call Cockburn Island; and, on the left, the strait which they fondly hoped was to lead them into the great ocean. On attempting, however, to enter this passage, it was found, to their deep disappointment, closed by a continued and impenetrable barrier of ice. In the course of the summer they worked their way forward ten or twelve miles, thus placing themselves within the strait, but never could reach farther; and the whole extent of the bay and of the sea appeared covered with ice piled upon ice, in immoveable masses. All their subsequent information was gained by land-expeditions, which enabled them to delineate the strait, its islands, and its opening, into the Polar Sea; but all their endeavours to penetrate across the great masses of continent were baffled by rocks, ravines, lakes, and inundations occasioned by the thaw. To the great tract of land on whose eastern cosat they sailed, they gave the name of Melville Peninsula.

On the commencement of the third summer, as their stores and supplies were drawing to a close, Captain Parry formed the too daring design of taking those of the Hecla into his own ship, sending home Captain Lyon, and prosecuting the voyage alone. Luckily this hazardous scheme was frustrated by the appearance of very formidable symptoms of scurvy, which left no choice but that the whole should hasten homewards.

In science and natural history, only slight and casual observations have as yet been given, these subjects being reserved for a separate publication, which shall be duly noticed. Of the human occupants of this territory, however, ample details are found, particularly in the very amusing journal of Captain Lyon, in regard to which the world is certainly much indebted to Mr Barrow, for rescuing it from the prescription in which expeditionary etiquette would have involved it. Our limits must now dictate to us great brevity. The inhabitants belong to the race of Esquimaux, who also occupy Greenland, Labrador, and all the bleak and outer corners of America. Their stature is short, and their visage appears decidedly to present the Mongol features; round and flat surface, eyes turned down at the corners,

and high cheek-bones; this last so remarkably, that a ruler might, in some cases, be applied, from cheek to cheek, without touching the nose. Their complexion is dark; yet, when divested of its usual thick coating of grease and dirt, it presented often an agreeable bloom.

In the domestic economy of the Esquimaux, the most remarkable feature consists of their winter huts, built entirely of snow. The snow is formed into blocks, which, laid over each other, and gradually bending inwards, terminate in a regular dome, sometimes nine or ten feet high. A plate of ice forms the window. When clusters of these huts have had their intervals filled up with snow and drift, they cannot be distinguished from the surrounding plain, and may be walked over; hence the idea of Greenland subterranean habitations; but when the roof is thinned by thawing, a leg is apt to come down through it. The entrance is long, and under ground, as described by Scoresby. In the interior, raised benches of snow, covered with skins, serve for sitting or sleeping on. Heat, light, and cooking, are afforded by one lamp, having a wick eighteen inches long, fed with oil or blubber, and which, when lighted through its whole length, makes a most brilliant and beautiful flame. Close to it, the temperature is raised to 38°, but in receding falls to 23°, and cannot be raised higher without the danger of melting this frail mansion. In spring, indeed, the dripping causes much inconvenience, and brings on severe colds. These mansions, however, are much more comfortable than those roofed with skins, the heat and closeness of which produce very bad effects.

The Esquimaux, however inured to the climate, seem never to become insensible to its influence, against which they guard by casing themselves in multiplied folds of deer-skin. The boots of the women in particular, resemble huge sacks, and give them the appearance of fat Muscovy ducks. For food, when the weather permits, they seek on land the rein-deer and the musk-ox; at sea, the salmon, and some other fish; but, during the greater part of the year, their dependence must be upon those large animals, the walrus and the seal, which being obliged to come above the ice for respiration, are thus exposed to their attack. They are liable to extremes of want; but after a successful hunt, they revel in abundance, and eat most

enormously. The quantity eaten in one day by a boy on board one of the ships, on being carefully weighed, was found to amount to ten pounds; and an allowance of half that quantity was considered by the sick complete starvation. The habit of eating only fat and oily flesh seems to cause a strange perversion of taste. Gingerbread, coffee, and other European delicacies, were taken, out of mere compliment, without any relish; but when they were presented with tallow-candles, or the soap with which the officers were washing themselves, these dainties were eaten with every symptom of intense gratification. This regimen produces a very plethoric habit, which, though relieved by habitual bleedings at the nose, subjects them to fevers and other fatal disorders. Dirt prevails in every shape, in their persons, houses, clothes, and all things belonging to them, or within their reach.

Although the Esquimaux are destitute of all abstract ideas, and cannot count above ten, yet their intellect acquires considerable development in the course of a roving life, where even the most common means of subsistence cannot be obtained without peril and adventure. In chasing and entrapping the various species of wild animals, they shew the greatest ingenuity and active courage. Their migratory habits give them much local observation; and their conversation by no means shewed any want of reflection and curiosity.

The Esquimaux exist in that state of society which is prior to any political union, except that formed by the mere junction of a few families, united by a sort of patriarchal tie. They are happily untinctured with that ferocity which characterises almost every other form of savage life. Though always bearing deadly weapons in their hands, they are quite strangers to war, never fight, and seldom even quarrel. Their domestic intercourse is cordial and cheerful; and they practise, without reserve, the duties of hospitality. They are accused, however, of considerable apathy, particularly in regard to their suffering friends or neighbours. Though sons admit the obligation to support their parents, they do not fulfil it with much care or attention. Widows, who cannot procure a second husband, are exposed to want, and even starvation. Their mourning for deceased friends is short and superficial; and as it is difficult to dig a secure grave in the frozen soil, the devouring of the flesh

by dogs and wolves is viewed with great resignation. The British charge them with want of gratitude for favours received, though they admit that the strictest honesty in the first instance marked their proceedings, and that it was not very extensively impaired. Of the fair sex, it is alleged that gossip and detraction are as favourite amusements as in the most civilised coteries; and that the outward propriety which marked their demeanour was not always strictly adhered to in private; but perhaps implicit faith is not always to be placed in the scandalous chronicle of the frozen regions.

The religious ideas of the Esquimaux are very rude. They have *angekoks*, or magicians, who impose on the vulgar by wild chaunts and strange contortions, and are supposed to hold influence over ten superior beings. Among these, the most prominent are, a huge bear, and a lady with one eye, who hold sway over the wild animals which are the objects of chace, and can grant or withhold them at will, or at the intercession of the angekoks. Like every other race, they anticipate a state of future bliss, though entirely of an animal and sensual nature,—a subterranean paradise, whose summer is always bland, lamps always filled with oil, walruses and seals in perpetual plenty. They did not appear to have any idols, or any outward form of worship.

We shall conclude with some observations on the prospects of success awaiting the New Expedition which has just set out.

It is impossible not to admire the determination displayed by Government, and by its Officers, in not allowing themselves to be dismayed by the imperfect success of two such hard naval campaigns; and we entirely concur in the propriety of making at least one final effort. At the same time, we are with pain obliged to state, that our anticipations as to the result are not very sanguine. It seems admitted on all sides, that there is no longer a hope of establishing a smooth and regular commercial route across these northern waters. This, indeed, leaves the enterprise, considered as a voyage of discovery, still highly interesting. But our auguries are unfavourable, even with respect to a single ship, ever, in a single instance, completing the voyage from one end to the other. Not but that there appears a very fair prospect, that, ere many years elapse, the

meritorious and indefatigable efforts of the British Government will have completely explored this northern boundary of America, but it will be done in a different track, and by different instruments. It will be the result of the efforts of the land-expeditions about to proceed and commence their career in successive points of this extensive coast.

Captain Parry's present idea seems to be, that his main hope is by keeping close to the coast of America, and taking advantage of the occasional openings which occur, when the ice, loosened by spring, is drifted out into the open sea. If this view be correct, the passage can scarcely be considered as at all possible. Recollecting how tedious and obstructed have been the short voyages hitherto accomplished in one season, it seems inconceivable that such an immense coast as that reaching to Behring's Straits, can be traversed without endless delay, and without meeting more than once with impassable barriers. In regard to the theory itself, however, though our diffidence must be peculiar in opposing our own opinion to that of Capt. Parry on such a subject; yet even his own data seem to afford ground to suspect, that the above impression may have been formed from his long continued habit of navigating in seas comparatively narrow, and inclosed by land. It appears clear, that the permanent ice, upon which, year after year, the summer heats are in vain exerted, is only that formed in or near land, in bays, rivers, and straits. The masses detached by the partial thawings of summer from the places of their original formation, are drifted into the centre of these mediterranean seas. Here, uniting from opposite sides, caking together, and annually accumulating, they form at length a broad mass of perpetual ice, which leaves only a narrow passage between it and the land. But the case seems likely to be very different with a wide ocean having no bordering land whatever. Such an ocean could have little ice, except that formed on itself, which is always of too soft and loose a texture to resist the heats of summer; for the partial and floating masses of the former ice, which might be wafted by wind and tide, could never combine into a permanent barrier.

In considering, however, the existence of such an open expanse of ocean as the only favourable case, it remains still doubtful whether it exists, and, yet more, what chance there is of ever reaching it. It appears ascertained, that there is a con-

tinued current setting through these seas, from west to east, which must carry in that direction all the loose masses of ice floating through them. These masses, being driven with peculiar force against the straits opening into such a sea, must, we fear, accumulate so as permanently to choke them up. This is evidently the cause of the huge mass of apparently perpetual ice, which bars the Strait of the Fury and Hecla; and the same cause seems likely to operate against every strait opening in a similar manner into the ocean. The only case in which this cause would not act, is, where the strait should open in a direction due north or due south, or even with a bend easterly; so that the current might pass by without entering: but we doubt if this be very probable. The general direction of Prince Regent's Inlet is indeed from north to south; but there is such a declination to the westward, as must probably expose it to the action of this perpetual ice-current.

For these reasons, there appears ground to anticipate, that the mode in which the northern coast of America will be delineated, is by land-expeditions. Government, accordingly, as we learn from the Quarterly Review, has three in contemplation. One, under Captain Lyon, is to proceed from Repulse Bay to the mouth of Hearne's River; another, under Captain Franklin, from Mackenzie's River to Behring's Straits; while Dr Richardson, who accompanies him to the first point, proposes to trace the way back to Hearne's River. When we consider, indeed, the various obstacles which beset the traveller in this unexplored region of rugged mountains, broad lakes, and winding shores, we cannot expect that even these enterprising individuals will effect any of the above objects in a single season; but we doubt not that every thing will ultimately yield to their persevering exertion. From this point also, expeditions in barks, or over the ice, might explore the insular tracts of this great ocean, as effectually as could be done by large vessels. It should seem necessary, indeed, to support these operations by a depot of some magnitude formed at the mouth either of Hearne's or Mackenzie's River.

It may be expected that we should here notice Captain Cochrane's suggestion of commencing the voyage of discovery at Behring's Strait. It has certainly the advantage, that the

navigators would then have the current in their favour; but this appears to us quite insufficient to balance the heavy objections against it. Before beginning their career, there would be a voyage to be performed nearly equal to the circumference of the globe, without any hospitable port to receive or refresh them. The straits, blocked up with ice, would be equally inaccessible on the western as on the eastern side; and if, as appears probable, the greatest difficulties are at the eastern extremity, this would only render more serious the situation of a vessel which, after a voyage of two years, had arrived there, without being able to make its way into Baffin's or Hudson's Bays. We know not how exactly to estimate the value either of the aid or opposition of this current; but it does not appear that Captain Parry, who had constantly to struggle against it, complains of any very formidable obstruction which it opposed to him; or that, when wind favoured, and ice did not oppose, there was any difficulty in making even a rapid progress. We see no reason to think, therefore, that the favour of the current thus gained would afford any compensation for the very high price at which it would be necessary to purchase it.

Since writing the above, we have perused the pamphlet published under the signature of *Scrutator*, entitled, "Impracticability of a North-west Passage for Ships." It displays considerable shrewdness and information; but the author writes in so caustic and controversial a tone, and employs himself so much in combating, now this, and now that passage, in the Edinburgh and Quarterly Reviews, that we have difficulty in making out any thing very precise as to his own views. It appears, indeed, that the passage, which we only think rather improbable, he thinks impossible. He disputes the existence of the great circumvolving current passing through Behring's Straits, and round the northern coast of America. The current through Behring's Straits, however, is attested by Kotzebue and Cook, and seems to arise almost necessarily from a conformation of coasts, similar to that which pushes the gulf-stream through the Bahama channel. How far the current from the westward encountered by Captain Parry can be identified with that coming form Behring's Straits, may admit of controversy. But if we

allow the existence of continued sea, which is surely at least more probable than the opposite supposition, the non-existence of this continued current from the west, would be a favourable circumstance to the navigator sailing westward.

The most important element which Scrutator has introduced into the calculation, appears to be the great preponderance of northerly winds in these latitudes, and the consequent descent of the polar ice upon all coasts having a northern exposure. This is likely to seal up all straits, sounds, and channels, opening in that direction. It may probably be admitted also, with our author, that the progress made by Captains Parry and Franklin may be in a great measure ascribed to their having, on the north, chains of islands protecting them from the arrival of these masses of polar ice. But there is nothing impossible, or even improbable, in the supposition, that similar chains may stretch much farther, and even to the vicinity of Behring's Straits; and, if Captain Parry could work his way from one to the other, a chance of success would arise not hitherto adverted to. If deserted by these insular barriers, the unfavourable omens already formed, respecting a progress along the coast of America, would, no doubt, be strengthened by this new view of the subject. Yet if the navigator could then push out into the open sea, although he might encounter floating ice-islands, we have no idea that, in a latitude little above 70°, he could find an ocean covered over with impenetrable ice.

Upon the whole, then, though the chances may be heavy against Captain Parry's present success, there seems no ground for considering, with Scrutator, the undertaking as desperate.

Additional Information regarding the Arctic Expeditions.

[The following additional particulars regarding the Arctic Expeditions, will serve to complete the view given by Mr Murray in his interesting memoir.—Edit.]

Captain Parry is to attempt to get through Prince Regent's Inlet, and to push for the coast of the American continent, which he hopes to reach somewhere about the Coppermine River. When he explored this inlet before, he was obstructed only by floe-ice, through which, in a favourable season, a passage may be found; and the general impression at that time was, that the land on both sides of it consisted of islands,—an opinion

which has since been confirmed. Many channels doubtless exist amongst these islands, though at the time the search was abandoned in that quarter, they were blocked up by the prevalence of a northerly wind. If the more favourable appearance of the passage between Prince Leopold's Isles and Maxwell Bay had not taken them away at that time, perhaps a passage to the southward might have been found. The ice was moveable. We agree, however, with Scrutator in thinking, that the North American continent would be much more easily coasted from west to east, than in the opposite course; for when a vessel gets involved in the ice, she must follow the current, and might thus get through many straits, which are quite impassable in the opposite direction. The only objection seems to be the length of the previous voyage before entering the ice. As to the hope of success, it depends much upon chance. If Captain Parry succeeds in getting past the barrier formed by the chain of islands running from Melville Peninsula to the west side of Regent's Inlet, and enters the open sea about Point Turn-again, we have no doubt of his success. It would appear from Dr Richardson's view of this subject, that the chief obstacle seems to be, as it were, in the threshold of the passage, where the difficulty, from the narrowness of the channels filled with ice brought down from the whole north coast by the current, is very great to a vessel proceeding to the westward *

Captain Lyon is to anchor his vessel in Repulse Bay, and, carrying his boats and provisions across a peninsula supposed to be about forty miles broad, to commence his voyage along the coast next summer. We fear he will meet with some obstruction on first embarking, from the quantity of ice which will be set into the *bight* behind Melville Peninsula by the current. Still we expect he will find a passage for a boat close to the shore, carrying it of course occasionally across projecting points. The only objection to this mode of proceeding, is, that it will cause him to coast all the inlets, instead of cutting across their entrances, and he will thus be compelled to waste that time by the ice, which Captain Franklin and Dr Richardson were ob-

* The cause of obstruction mentioned above, is noticed by Dr Richardson at page 500. of Captain Franklin's Voyage.

liged to do from the slenderness of their vessels. When he has once succeeded in getting out of the bight, his task will be easy. The delay in this case will be in starting, and these seas are not open for boat-navigation more than six weeks at furthest. After reaching *Point Turn-again*, he returns to his ship. If he can make a tolerable straight course, his voyage along the coast and back again will be about 800 miles.

Captain Franklin is to proceed in *boats* along the coast from Mackenzie's River to the westward, towards Behring's Straits. His arrangements have been made with great care. Depots of provisions are to be established near the sea, and we feel confident of his safety and success. Dr Richardson leaves Captain Franklin at Mackenzie's River, with the view of examining, in all its details, the natural history of the country extending eastward to Coppermine River, and probably farther,—an investigation which cannot fail to procure for this distinguished traveller additional claims on the gratitude of the scientific world.

The schemes that have been proposed of using rein-deer, dogs, &c. are quite Utopian. Sledges can only travel in the winter season. Captain Franklin and Dr Richardson found, that when a journey exceeded twenty days, the dogs could drag little more than their own provisions. Rein-deer are not yet domesticated in America. Years must elapse before that is effected, and it would be easier to begin by civilising the natives who inhabit the parts required to be surveyed. As to establishing depots of provisions from Canada or England, the distance they require to be carried by land, in either case, would, as the crow flies, exceed 2000 miles. A canoe fully laden carries 30 pieces, including the mens' bedding, and the provision for the crew for four months. The shortest time the voyage would occupy from Canada to the depot, at 3 lb. a man per day, would amount to the whole freight, leaving them to depend upon the resources of the country for the remaining eight months. If supplies of provision were not obtained from the immediate posts, none of the trading goods could be carried up to Mackenzie's River. It requires, however, one, and in some cases two years' notice to receive these supplies, and their amount is limited.

ART. II.—*Abstract of an Account of Crystallisations formed during various Metallurgical Operations.* By Mr FREDERICK KOCH of Königshütte in the Hartz.

[The interesting little work entitled *Beiträge zur Kenntniss Krystallinischer Hütten producte, Von F. Koch,* &c. from which we have abstracted the following particulars, was sent to us by the author, through our excellent friend Professor Hausmann of Göttingen. We have no hesitation in recommending it to the particular notice of the chemist and mineralogist. The crystals are described by Mr Koch according to the System of Hausmann; but this being little understood in this country, we have described them according to the method of Mohs, which is already known to the British public, through Professor Jameson's Article *Mineralogy,* in the Supplement to the *Encyclopædia Britannica.*]

A. OXIDES.

I. *Black Oxide of Iron.—Magnetic Iron-Ore.*

IN the process of converting crude into malleable iron, the carbon of the crude iron is destroyed. If the crude iron is free of all foreign intermixture, as of the metalloids, sulphur and phosphorus, it will, when completely deprived of carbon, appear as pure malleable iron. If, in this state, it is heated in a furnace, and exposed to the influence of oxygen, an oxide of iron is formed,—first, a black oxide, and by long exposure to this heat, a red oxide. During the conversion of crude into malleable iron, a considerable quantity of black oxide is formed, but which speedily melts and combines with the earthy matters, and forms the iron-slag. In the process of forming malleable iron from crude iron, the crude iron is melted and exposed to a high degree of heat. During this process, in favourable circumstances, black oxide of iron is formed, which is not distinguishable from magnetic iron-ore. This metallic product has all the properties of natural magnetic iron-ore. The colour is iron-black and steel-grey. It is strongly magnetic, but is not itself a magnet,—and is regularly crystallised.

The following varieties of this artificial magnetic iron-ore are enumerated by Koch :

a. Foliated magnetic iron-ore.

a. *Common.* Crystallised.

b. *Lamellar.* In larger portions, disseminated in compact magnetic iron-ore.

b. Slaggy magnetic iron-ore. Imbedded in the compact, resembling that of Unkel on the Rhine, which occurs in basalt.

c. Compact magnetic iron-ore. Massive.

The following crystallisations were observed by M. Koch :

1. The fundamental form, is the regular octahedron, perfect. Sign O, *Mohs.*
2. Octahedron, elongated in the direction of a prismatic axis.
3. Octahedron, slightly truncated on all the angles. Sign H, O, *Mohs.*
4. Cube slightly truncated on the angles and edges. Sign H, O, D, *Mohs.*

The crystals 1, 2, and 3. have a splendent metallic lustre,—the less perfect crystals a dull and rough surface, and the first sometimes exhibit a variegated tarnish. They are always grouped together in great numbers, and it is remarked that the crystals of the fundamental form are frequently grouped in rows. They seldom attain the size of a line. Crystals of this mineral are also formed where plates of iron highly heated, come in contact with a aqueous vapour ; and M. Nordenskiold informed Mr Koch, that artificial crystals of magnetic iron-ore are formed by the melting and gradual cooling of common magnetic iron-ore.

II. *Oxide of Zinc. Calamine.*

Hausmann, as mentioned in a former number of the Edinburgh Philosophical Journal, mentions this product as occurring in iron smelting furnaces: The following forms are described by M. Koch, which we enumerate according to the method of Mohs. The artificial oxide of zinc belongs to the prismatic system of Mohs. The fundamental form is described by Koch as an obtuse scalene four-sided pyramid, in which the ratio of

the diagonals $a : b : c$ in the fundamental form is $= \sqrt{17} : \sqrt{48} : \sqrt{16}$. Supposing p 24., P, Fig. 1. Pl. VIII. to be equal to the fundamental form, it will follow that the sign of

Fig. 2. is $\underset{a}{P - \infty}.\ \underset{P}{P}.\ \underset{o}{\breve{P}r + 1}.\ \underset{e}{P + \infty}.\ \underset{b}{\breve{P}r + \infty}.$ *Mohs*

Fig. 3. $\underset{a}{P - \infty}.\ \underset{e}{P + \infty}.\ \underset{b}{Pr + \infty}.$ *Mohs.*

Fig. 4. $\underset{m}{\frac{5}{4} P + 1}.\ \underset{s}{\frac{5}{4} \breve{P}r + 2}.$ *Mohs.*

Fig. 5. $\underset{a}{P - \infty}.\ \underset{m}{\frac{5}{4} P + 1}.\ \underset{s}{\frac{5}{4} \breve{P}r + 2}.$ *Mohs.*

The crystals are seldom single, generally many grouped together; occurs massive, in crusts, and in radiated concretions. Its colours are greenish and yellowish-white. It is semitransparent and transparent. The massive specimens, M. Koch remarks, bear a striking resemblance to the precious radiated calamine of Hausmann. These varieties were found at the iron-forges of Königshütte. It is also found crystallized, and exhibiting the following colours at the iron smelting furnace of Zorg; emerald, leek and oil green; honey, ochre, and straw yellow.

B. Earths.

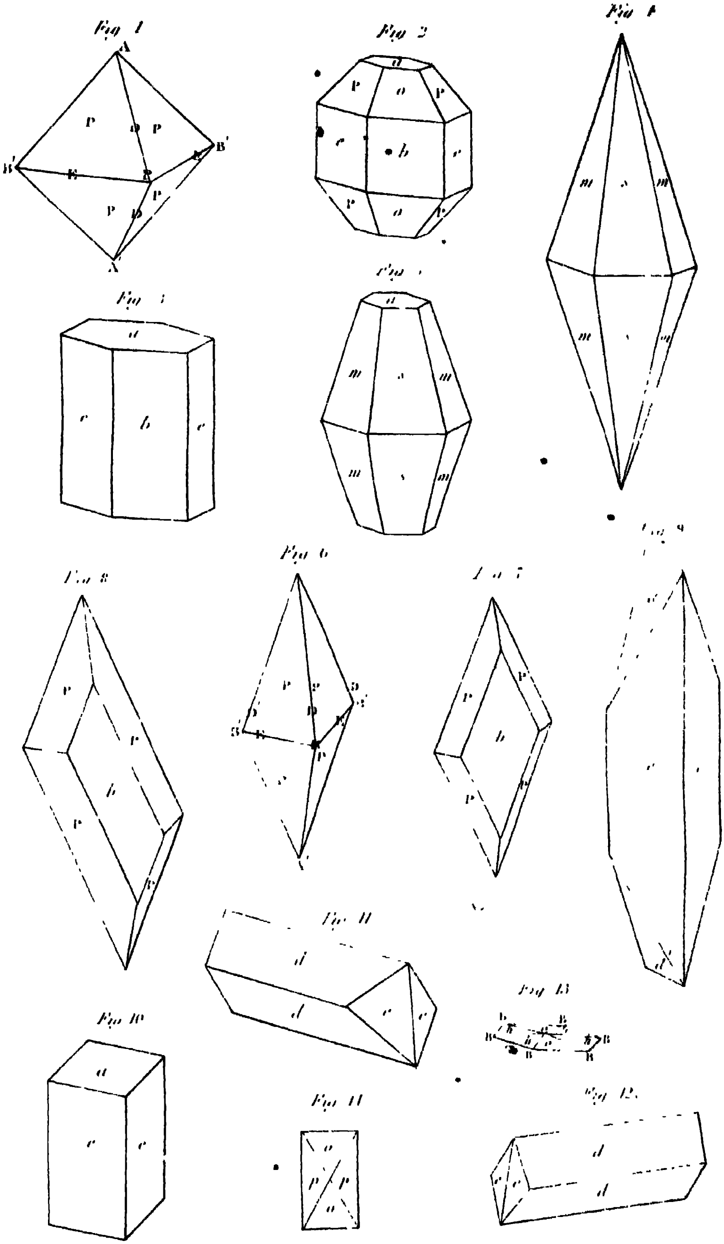
Silica.

Silica occurs in a remarkably pure state amongst metallurgic products, and exhibits the most beautiful and delicate forms. Its occurrence, as an artificial production, was first noticed by Grignon, who describes it under the name Iron-amianth, and Vauquelin first ascertained its true nature. Meyer, Quensell, Hausmann, and Stromeyer, were the first who enumerated and described it as a product of the forges in the Hartz. It is pure silica. Two principal kinds are described by Koch.

1. *Fibrous Silica,* which has snow-white and ash-grey colours. Its structure is fibrous, concentric and scopiform, with a silky lustre. Its forms are globular, botryoidal, and reniform. Is friable.

2. *Earthy Silica.*—Snow-white. Pulverulent, but becomes more and more compact, and passes into fibrous. Glimmering

PLATE VIII.



Figures of crystals formed by crystallization from fusion

and dull. Generally the fine earthy particles are irregularly aggregated, seldomer it is globular, reniform, and botryoidal. Both kinds are found in smelting-furnaces where siliceous iron-ores are smelted. It is sometimes associated with artificial tabular *graphite*.

C. Vitreous Slags.

Siliceous-enamel? (Kieselschmelz.)

This interesting production of the smelting-furnace is described by Hausmann, in Vol. V. of this Journal, but the description by Koch, of which we shall now give an abridged account, is much more complete. It appears as a perfect glass, and passes gradually into a kind of enamel. Some of the varieties bear a striking resemblance to glass, others approach more to enamel, and there is an uninterrupted transition from the one into the other. Two principal kinds are described:

1. *Glassy Siliceous Enamel.*—Is perfectly glassy, friable, transparent, or only semitransparent: fracture perfect conchoidal, and lustre vitreous; also small conchoidal, splintery, and uneven, and lustre resinous. Colours, pearl, greenish, and reddish grey. Crystallised. Massive.

2. *Opaque Siliceous Enamel.*—Very much like enamel. Alternate from semitransparent to translucent. Fractures uneven, even and splintery. Structure fibrous and radiated. Lustre resinous, glimmering, and dull. Colours white, grey, yellow, and blue. Crystallised, massive, and disseminated.

The following are some of its regular forms, as given by Koch, our space not allowing the insertion of the whole.

The forms of this artificial mineral belong to the Prismatic System of Mohs, and to the Trimetric System of Hausmann. The fundamental form is an acute scalene four-sided pyramid.

The ratio of the axis *a*, and the diagonals *b* and *c* is as $\sqrt{6} : \sqrt{3} : 1$.

Fig. 6. $P = 123° 54' 58'' : 70° 31' 44'';\ 141° 3' 28''$.

Fig. 7. P. $\breve{P}r + \infty$.
P. $\breve{b}$

Fig. 8. The same elongated in the direction of one of the obtuse terminal edges of P.

Fig. 9. $\bar{P}r. \quad P+\infty.$
$\quad d' \quad e.$

Fig. 10. $P-\infty. \quad P+\infty.$
$\quad a \quad e.$

Fig. 11. The same as Fig. 15., only elongated in a direction perpendicular to the former.

Fig. 12. Similar figure.

Fig. 13. Supposed to be $\frac{1}{3}P-4. \quad \frac{4}{3}\breve{P}r-3.$

Fig. 14. The sign of the faces p is $\frac{4}{3}Pr-3.$

These crystals occur either single or variously grouped. In general sharply bounded crystals seldom occur, the edges being more or less rounded, thus appearing as having been melted on the edges.

The siliceous enamel, Koch remarks, he has hitherto found only in the slag formed during the formation of common grey crude iron by means of charcoal. It is formed at the same time with the slag, and occurs in it in crystals, crystalline masses, and in larger and smaller particles, with a crystalline or curved surface, and sometimes the greater part of the slag is composed of siliceous enamel. Koch further remarks, that, for the formation of crystals, repose and slow cooling are necessary; that, in those cases where the slag is principally composed of the enamel, the most frequent varieties are the common and fibrous, and the fibrous is generally disseminated in the other: He further observes, that crystals, having an opaque and glassy character, sometimes occur in it: On the contrary, in those cases in which the slag predominates, the enamel occurs in it, in imbedded masses and crystals. Where the crystals are perfect, and with sharp edges, the slag is easily fusible: On the contrary, where the crystals are imperfect, rounded, without sharp edges and angles, the slag appears to have been more viscid and is difficultly fusible. These facts shew that the substance of the enamel is less fusible than that of the slag, and must have cooled sooner; consequently, that these crystals were consolidated before the slag in which they are contained. Koch, in a note, remarks, "Does not the occurrence of this enamel in a mass, having different properties from itself, throw some light on the

following query of Breislac, in the Introduction to his Geology, p. 451.," "Les cristaux contenus dans les laves se sont-ils formés dans le lave meme, et ont-ils été produits par les matieres qu'elle contenait, ou bien lui sont-ils etrangers; et etaient-ils formés anterieurement, preexistant dans les matieres et dans le couches prerreuses fondues par le feu volcanique?" Koch also remarks, that the glassy and opaque varieties of enamel pass into each other, and both varieties or kinds occur in the same crystal; and, what is of importance in a theoretical view, in crystals exhibiting these kinds, the glassy kind forms the centre of the crystal, while the opaque forms the crust or outer part; and a gradual transition from the vitreous centre to the completely opaque exterior is observed in such crystals.

Constituent Parts.

Silica, - -	56·40
Lime, - -	26·24
Alumina, - -	8·33
Magnesia - -	3·96
Oxide of Magnesia, -	1·81
Oxide of Iron, - -	0·18
	96·92
Loss,	3·08
	100·00

SALTS.

Muriate of Soda.

Alkaline salts, which are almost always formed from aqueous solution, sometimes appear along with metallurgic products, as the result of the action of high temperatures. Muriate of soda, we are informed by Koch, occurs in some smelting furnaces, not only in massive forms several inches in magnitude, but also beautifully crystallised in cubes, which are in druses, but seldom in single crystals. They are met with in fissures of the furnace, and appear to have been formed by sublimation.

ART. III.—*Account of the Parallel Roads in the Valley of Coquimbo.* By Captain BASIL HALL.

THE *parallel roads*, or natural terraces of Glen Roy, in this country, have, for some time, engaged the particular attention of

geologists, who, as usual, differ much in opinion as to their mode of formation. The most natural explanation is one we suggested many years ago, which refers the whole of the phenomena to changes connected with alterations in the level of a great lake, that formerly occupied the district where the parallel roads occur. Captain Basil Hall, in his highly interesting and delightful "Journal of a Voyage along the coasts of Chili, Peru, and Mexico," employs a similar explanation in the following account of parallel roads or natural terraces, he met with in the Valley of Coquimbo *.

"On the 18th November, our friendly host accompanied one of the officers of the Conway and myself, in a ride of about twenty-five miles, up the valley of Coquimbo, during which, the most remarkable thing we saw was several series of horizontal beds, along both sides of the valley, resembling the parallel roads of Glen Roy, in the Highlands of Scotland, so carefully examined by Thomas Lauder Dick, Esq., and described in the ninth volume of the Edinburgh Royal Society's Transactions. They are so disposed as to present exact counterparts of one another, at the same level, on opposite sides of the valley. They are formed entirely of loose materials, principally water-worn rounded stones, from the size of a nut to that of a man's head. Each of these roads or levels, resembles a shingle beach, and there is every indication of the stones having been deposited at the margin of a lake, which has filled the valley up to those levels. These gigantic roads are at some places half a mile broad, but their general width is from twenty to fifty yards. There are three distinctly characterised sets, and a lower one, which is indistinct when approached, but, when viewed from a distance, is evidently of the same character with the others. The upper road lies probably 300 or 400 feet above the level of the sea, and 250 from the bottom of the valley; the next twenty yards lower, and the next about ten yards still lower. These distances are loosely estimated, and may be erroneous, for it is difficult to determine heights or distances in a country quite new, and without natural and determinate objects of comparison. In this valley, there being neither trees, houses, cattle,

* There are beautiful displays of *parallel roads*, in some of the valleys through which the Rhine flows.—EDIT.

nor men, our estimates were made entirely by guess. This, however, does not affect the general question, but only the proportions. When at any time we found ourselves on one of these parallel roads, we saw, upon looking across the valley, or up or down it, as far as the eye could reach, portions of flat spaces, apparently on the same level with that on which we stood; and when, in order to determine this more exactly, we went over the edge of the road or beach, and brought our eye into the plane of one of the roads, we invariably found, on looking round, that the same plane produced would merge into every portion of the same road, exactly as we should see the margin of a lake, with all its windings, on a level with the surface, if, while bathing, we were to bring the eye close to the water and look round. I regretted not having time to return with a spirit-level, to examine this question of horizontality by infallible means.

In the centre of the valley, which is six or seven miles wide, we found an extensive plane, narrow at the upper end, and widening out towards the sea, thus dividing the valley into two parts. This insulated space, was, to all appearance, quite flat and horizontal, and, as far as the eye could determine, exactly on a level with the highest of the above mentioned roads; so that, if a lake ever stood in this valley, at the level of the road, the present surface must have been barely covered with water, or, as seamen term it, just lipping with the water's edge. It is several miles wide, and shaped like a delta; its sides are at many places deeply indented with ravines, which show it to be composed exclusively of the same water-worn materials as the roads; and on both sides, the roads are easily traced at the same levels, and in perfect conformity with those on the opposite banks of the valley. The stones are principally granite and gneiss, with masses of shistus, whinstone, and quartz, mixed indiscriminately, and all bearing marks of having been worn by attrition under water.

The theory which presents itself to explain these appearances, supposes a lake to have been formed, no matter how, and to stand at the level of the highest road, till a flat beach is produced by stones being washed down from above. The water in this lake is next conceived to wear away, and break down a portion of the barrier; this allows the lake to discharge part of its waters into the sea, and, consequently, lowers it to the second level;

and so on successively, till the whole embankment is washed away, and the valley left as we now see it.

The stones all bear the marks of having come from some distance, and may possibly have been deposited by a river flowing from the snowy Andes in ancient times; while some vast, though transient cause, may, at one operation, have scooped out the valley, filled it with water, and left a barrier of adequate strength to retain it for a time, till, by a succession of sudden disruptions of this barrier, the lake would stand at different levels, and the washing of the water down the sides of the banks, would bring along with it the loose stones to the water's edge, where their velocity being checked, they would be deposited in the form of level beaches. In the Alpine valleys of Savoy, circumstances precisely analogous frequently occur, when a great avalanche dams up a stream, and forms a lake, which stands at different levels, as the barrier of ice successively breaks away *."

ART. IV.—*On Thermo-Magnetism.* By THOMAS STEWART TRAILL, M. D., &c.

[The following is the substance of a Paper which was read at two successive meetings of the Royal Society of Edinburgh, on February 2., and February 17. 1824.]

IN the first part, the author detailed a long series of Thermo-magnetic experiments. The apparatus he employed consists of bars of antimony, bismuth, &c. 4½ inches long, ½ inch broad, and ¼ inch thick; to which slips of copper, of the form exhibited in Plate X, Fig. 3. are united, by tying their extremities to the ends of the bars with fine copper-wire. This renders soldering unnecessary; and, when the thermo-magnetism is diminished by the oxidation of the surfaces in contact, they are soon brightened by rubbing them with sand-paper, or a fine file, and easily replaced.

This form of the apparatus was found very convenient for examining the thermo-magnetism of every surface of the compound metallic rectangle; and the influence of different posi-

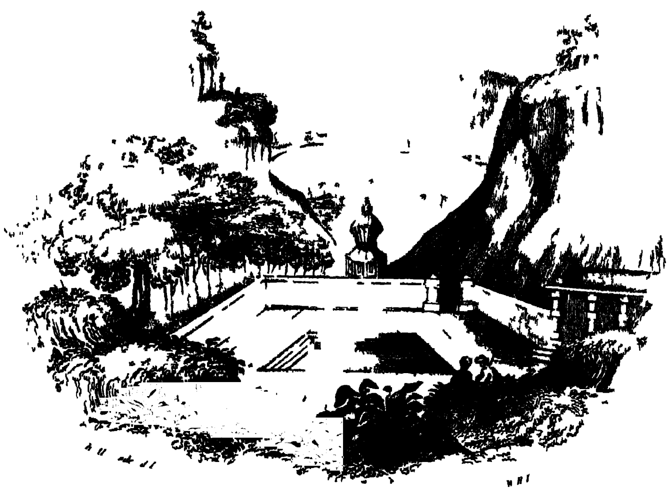
* We observed a series of natural terraces along the sides of one of the basins in the line of the Union Canal. We reckoned four or five of these, which were formed by the successive lowering of the water in the basin. EDIT.

PLATE X.

Fig 1

Fig

Fig 4



VIEW OF THE SALT LAKE OF LOONAR

tions of the heated end, with regard to the magnetic meridian, was with facility ascertained, by laying it on a small wooden stand with a revolving top; so that by a single application of a spirit lamp to one of the extremities of the apparatus, its thermo-magnetism in different positions could be easily determined.

The following are the general results of the experiments detailed in this part of the paper.

1. Of all metals, antimony forms the most powerful combination in thermo-magnetic experiments. 2. The deviation of a finely poised needle, was in general greater when placed *within*, than without the rectangle. 3. In any one position of the heated extremity, the direction of the deviations is the same on any one of the inner surfaces of the rectangle; and was uniformly in the opposite direction on any of its outer surfaces. 4. It is not necessary to place the support of the needle *in contact* with the metals. A suspended needle, or the interposition of a thick plate of glass, did not prevent the deviations caused by any of the surfaces. 5. The *upper surface* of the bar of metal produced deviations in the same direction, whatever may be the relative position of the connecting piece. 6. The direction of the deviation is different when opposite extremities of the apparatus are heated. 7. In the same relative position of the two metals, and on their similar surfaces, the points on each side of the meridian which give the greatest deviations, are exactly opposite to those which give the least. Thus, if the heating of the W. end of the apparatus give a deviation equal to 180°, then the East end heated will not alter the direction of the needle within the rectangle, and *vice versa*. 8. The extent of the deviations in all cases depends not only on the nature of the metallic bar employed, but also on its position with regard to the magnetic meridian. When the apparatus lies at right angles to the magnetic meridian, the deviations are the greatest. With antimony, in this position, the needle was completely inverted; but, for this effect the needle must be placed *within* the rectangle when the W. end is heated, and without it when the E. end is heated. 9. Experiments with the bars of antimony, bent at right angles, and heated at different points, shew that the direction of the magnetic deviation did not

depend on the position of the *initial point* of the disturbance of the equilibrium of temperature, but on the direction in which it arrives at the needle. 10. A bar of bismuth, under all circumstances, gives opposite deviations to one of antimony. An apparatus constructed with a bar of the former metal, was of considerable power; but the low melting point of bismuth renders it less suited to such experiments than antimony. 11. Various metals affect the needle as antimony, others as bismuth: Among the former are, silver and copper, zinc and copper, iron and copper: Among the latter are, platinum and copper, platinum and silver, lead and copper, brass and copper, Chinese tutenag and copper, &c. 12. When a piece of metal has one of its surfaces applied flatly to another equal metallic plate, a thermo-magnetic combination is formed; less powerful indeed than when united as already described. Even a speck of solder on a piece of copper appears to produce this effect. 13. Dr Traill was unable to form any thermo-magnetic combination of *one metal alone*, when the pieces united were of equal purity; but he found that two different copper wires, of unequal purity, gave slight indications of thermo-magnetism *. 14. Nor did he succeed in forming such combinations with one metal and water, nor with a metal and pieces of stony matter, though he seems to think, that a nearly neutralized needle might enable us to detect thermo-magnetism in large masses even of different kinds of stone.

The very remarkable effect of helical connecting pieces in electro-magnetic combinations, induced Dr Traill to try their thermo-magnetical influence; and he found a striking analogy between their effects in both cases; the relative position of the zinc plates to the copper, in the former class of phenomena, producing similar effects on the needle as heating either extremity of the bar of antimony in the latter. A horizontal right helical connecting piece produces, with a bar of antimony, inversion of poles, when the N. end of the apparatus is heated;

* The author finds, that, though he was at first unable to produce thermo-magnetic effects by heating a *single metal*, yet the experiment of Bequeret taught him, that they could be shewn by heating the joined wires of a nice galvanoscope. This does not alter any of the hypothetical deductions of the memoir, but even renders the thermo-magnetism of the earth more easily conceived.

and a half helix inversion when the S. end is heated; in electro-magnetism, when the zinc is to the N. of the copper, they are connected by a right helix inversion of poles taking place; while the same effect is produced with a left helical connecting piece, when the zinc is to the S. of the copper. The result of various experiments shewed, 15. That the needle is more powerfully acted on when it is in the axis of helical connecting pieces, than when the connecting pieces are simple slips or wires of metal. 16. The influence of right helices are the reverse of those of left helices in thermo-magnetic, as well as electro-magnetic combinations. 17. The effects of inclined and vertical helices in thermo-magnetic combinations, shewed that helices tend to cause the needle to arrange itself in the direction of the axes of the helix; but the right vertical helix caused the S. pole to dip; the left vertical helix depressed the N. pole of the needle suspended in its axis. 18. All the phenomena tend to shew, that a thermo-electric apparatus becomes a real magnet; and this idea is confirmed by Professor Cumming's little apparatus, which is attracted and repelled by opposite poles of a magnet. 19. The disturbance of the equilibrium of temperature in thermo-magnetic combinations is the cause of their influence; for a piece of ice applied to either end of the rectangle produces effects just the reverse of those which follow the application of the spirit-lamp to that extremity. 19. The application of ice, or of the lamp, to the centre of the antimony, produced opposite deviations in two needles placed between the centre and extremities of the bar.

Dr Traill thinks, that the phenomena of thermo-magnetic combinations favour Ampere's idea of magnetism being produced by currents moving in opposite directions; and he shewed how these might be represented, by lapping a fine wire, as a left helix, round the sides of an antimonial thermo-magnetic combination, and as a right helix around one composed of bismuth and copper, commencing the operation at the end supposed to be heated.

The second part of the paper was on the application of thermo-magnetism to some of the phenomena of terrestrial magnetism. The author conceives, that the disturbance of the equilibrium of the temperature of our planet, by the continual ac-

tion of the sun's rays on its intertropical regions, and of the polar ices, must convert the earth into a vast thermo-magnetic apparatus. From the time of the celebrated Dr Gilbert, the magnetic poles of the earth have been considered as "the collected powers of all the magnetic *ferruginous* substances" which enter into the composition of our planet: but thermo-magnetism teaches us the important fact, that other metals as well as iron, under certain circumstances, are capable of acting on the magnetic needle; and the analogy of the effect of mere position in rendering buildings, and other vertical objects, magnetical, would incline us to believe that the disturbance of the equilibrium of temperature, even in stony strata, may elicit some degree of magnetism. Be this as it may, there can be little doubt, that the unequal distribution of the earth's temperature must have some effect of modifying its "*magnetism of composition.*" Dr Traill considers the direction of the needle as the resultant of two forces, the magnetism of composition of the earth, and its thermo-magnetism. The direction of these forces do not appear to be the same. The evident tendency of a thermo-magnetic apparatus, is to place a needle exposed to its influence at right angles to the axes of the apparatus, or at right angles to the direction in which the inequality of temperature is propagated. If the earth be a thermo-magnetic apparatus, its thermo-magnetism, in like manner, must tend to place the needle W. and E.

The author endeavoured to shew, that the best established oscillatory movements of the needle were connected with changes in the earth's temperature; and these he adduced as proofs of the thermo-magnetism of our planet. He further conceived, that the varying declination of the needle could be more satisfactorily explained on thermo-magnetic principles, than on any other hypothesis hitherto offered. He considers the existence of two poles of greatest cold in either hemisphere, established by a comparison of actual registers of temperature, as generalised in the isothermal lines of Humboldt; and history, tradition, and geological investigation, all concur in proving that the isothermal poles have not been stationary. The short period to which accurate thermometrical registers reach, prevents us having any certain data on which to ascertain the relative tempe-

rature of Europe in remote ages; but the Greek and Roman descriptions of the climate of south and of central Europe 1700 years ago, are so strikingly different from our experience, that some cause, more powerful than the feeble efforts of human industry, is required to explain the change which has taken place; and the migration of the isothermal poles has been strongly insisted on by Dr Brewster, as the chief cause of our improved climate. Dr Traill maintains the same argument, and endeavours to shew, that the accumulations and disruptions of the Greenland ice, and the coldness of ancient Europe, would appear, as far as our imperfect knowledge of magnetism in past ages will permit, to have a remarkable connection with magnetic phenomena.

Art. V.—*Notice of a mode by which a conjecture may be formed as to the Sex of the Chick in Ovo.* By Mr David Ritchie.

In physiology it is interesting to ascertain at what period the embryo assumes a sex, and by what means its sex may be distinguished. In regard to viviparous animals, such inquiries are attended with much difficulty, as accurate dissection and minute microscopic observation are necessary. Such investigations, too, cannot be carried on without cruelty. Hence it is that physiologists have so often and so carefully observed and described the embryo in oviparous animals, from its first appearance in the egg till its complete development. For here the embryo may be subjected to examination without injury to the parent, and there is such similarity in the mode of growth of animals in the one case and in the other, that any fact ascertained as to the chick in the egg has usually led to a similar discovery in regard to the fœtus in utero. The appearances presented by the egg have been described by the most celebrated anatomists of our own and of other countries. I shall advert to the opinions which have been entertained by them as to the egg, only in so far as they relate to my present subject.

Eggs of the same bird differ from each other in shape. It has been asserted that the longer eggs contain males, the shorter ones females. To determine this, experiments were lately made

by Dr Autenreith *jun.* of Tubingen. From his experiments it appears, "that the opinion so generally entertained by his countrymen, that cocks are contained in the longer eggs, and hens in the shorter and rounder ones, is without foundation; for the sex is indicated neither by the shape nor by the size, nor in any way by the external form of eggs *."

A French naturalist, Bory de St Vincent †, speaks of a physician who had examined many thousand eggs, without being able to ascertain, from their external appearance, whether they contained cocks or hens.

Eggs of the same bird differ from each other in specific gravity. It has been conjectured, that the heavier eggs may contain males, the lighter ones females. But it is now generally known that eggs lose weight by being kept; and it has been shewn that this diminution arises from "the substitution of air for a portion of the water of the egg which escapes ‡."

Eggs were found by Dr Prout to differ from each other in the relative proportions of their saline principle. "I have sometimes thought," says he, "these differences, as well as some other singular ones observed with respect to the earthy matters, might be connected with the sex of the future bird:" but he adds, "no proof of this could be obtained §."

I have heard of a mode of discovering the sex of the chick in ovo, different from any which has been proposed by naturalists. The folliculus aëris, or air-cell, which is to furnish oxygen to the future chick, is situated at the larger end of the egg. It has not in all eggs the same position at the larger end; and in various districts of Scotland, it is believed that eggs having the air-cell situated *exactly* at the top of the larger end, produce males; while those having the air-cell only *near* the top of the larger end produce females.

To ascertain this, I instituted a series of experiments. These experiments, as will be seen, go very far to prove, that the opinion which has been stated is correct, and so to determine what naturalists of Germany, France, and England, have endeavoured in vain to discover.

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* *Dissertatio Inauguralis*, p. 26. Tubingen 1821.

† *Voyage de St Vincent*, p. 64. Paris 1804.

‡ *Phil. Trans.* for 1822, p. 378.

§ Ibid. p. 386.

Eggs were selected, all having the air-cell situated exactly on the apex of the larger end, and were hatched. In most instances, the birds were all males.

Eggs were selected, all having the air-cell situated, not on, but near to the apex of the larger end. In most instances, the birds were all females.

These experiments have been frequently repeated, and were conducted for me by persons residing in different parts of the country.

A few of the experiments were not quite so satisfactory as the others. Among a dozen of birds of one brood, two or three turned out to be of a different sex from the rest. I may remark, however, that it is sometimes rather difficult to determine whether the air-cell be exactly on the apex or not; and the anomaly may have originated in error arising from this cause.

Every person has observed the air-cell in the boiled egg upon removing the shell: in the recent egg it may be perceived by holding the egg between the eye and a lighted candle. In general even an unpractised observer may determine the position of the cell, whether it be at the axis or some little way from it, and so conjecture the sex of the future bird. Occasionally, however, as already remarked, it is rather difficult to decide whether the air-cell is or is not on the axis. Such eggs should not be used by those who may be induced to put the criterion to the test of experiment.

The fact ascertained by these experiments seems to deserve the attention of the economist and of the physiologist.

At present I would only remark, that the result militates against an opinion, which has been ably defended, in regard to the ova of quadrupeds, and which has been advanced, too, as to the eggs of birds, That previous to impregnation, they have no distinction of sex, but are so formed as to be equally fitted to become male or female; and that it is the process of impregnation which marks the distinction.

The air-cell exists in unfecundated eggs. In some of these it is situated at the top of the larger end; in others near the top. From this it would appear, that the male parent does not influence the sex of the embryo, but that the ovum has assumed its sex previous to impregnation.

construct wholly of iron, to be covered on the top with a coat of gravel or road metal, four inches thick: this will soon consolidate, and form a smooth, soft, and permanent footing for the horses, and the iron below being preserved by it from the action of the external air, will continue for a long period without requiring any renewal or repairs. The gravel is laid upon a series of iron-plates, united together into one continued sheet of iron, extending quite across, and from end to end of the bridge; these plates again being laid on and firmly supported by a strong body of malleable iron frame-work, running every where below them. The two footways are raised six inches above the carriage way, and are divided from it by a ledging, along which there are placed, at convenient intervals, a series of short iron posts, to prevent carriages driving on the foot-paths. The foot paths, again, are terminated and guarded on the outside, each by a strong double railing of iron. The whole roadway, consisting thus of frame-work, iron plates, railing, and gravel, will form, together, a mass of such weight and breadth, and so strongly bound together in every part, that it will prevent entirely that lateral motion to which such bridges are usually subject: it will present such a mass of solidity and stiffness, that the highest wind will have no effect on it, and even the striking of a vessel will do no serious damage, for the bending of the roadway, and the yielding of the great mass of chains, will soon effectually check the motion of the largest vessel that can ever strike here, and that long before the action could have any sensible effect to move the suspending pillars. But besides that motion which takes place in suspended roadways from side to side, we must equally guard against any vertical undulation to which such a roadway may be subject. This is effected in the present bridge,—1st, By extending under the roadway a series of malleable iron-plates, running, longitudinally, from end to end. These plates, standing vertically, both support the iron-plates above, and give to the roadway a degree of stiffness up and down, as the horizontal plates stiffen it from side to side: this effect also is greatly aided by the two double side railings above it, and which run along the foot-paths. 2d, By binding the roadway very firmly to the chains, and binding these latter all together into one great mass or cable of chains, so that any

load or heavy body moving along the bridge, acts, not on one, but on all the chains together, and the effect being thus distributed over a greater mass, is much less sensibly felt. The roadway is suspended, or bound to the chains, by a series of iron rods, hanging vertically; and I propose besides, 3dly, To extend a similar series of rods horizontally, from the chains to the suspending pillars, so that these horizontal rods, interweaving with those which hang vertically, the whole space directly under the chains, and between them and the roadway, will be filled up by a kind of net or wire work, on a large scale, and which, without interrupting the view, will yet communicate a remarkable firmness and steadiness to the roadway, the reason of which may be easily shewn, and has indeed been experienced in the model which I have constructed. If these and other precautions are adopted, every unpleasant undulation in the roadway will be prevented.

Since there is no intermediate bearing for the roadway between the opposite main chains, it is evident that we have a stretch of 30 feet for our flooring, and that if we were to construct it in the common mode, with beams and planking, it would require extraordinary weight and strength of materials to withstand the cross strain arising from such an uninterrupted span between the opposite supports. We must here, therefore, adopt the principle of the roof, or something equivalent to it. I propose to sustain the whole weight of the roadway on a series of malleable iron-bars, stretched at intervals of five feet between the opposite sides of the roadway, and each hanging down three feet in the centre, in the form of a suspended arch, the extremities of these bars forming the points of suspension, and being attached to a cast iron socket, to which are also fixed the lower extremities of the suspending rods of the bridge; so that, while the suspending rods bear up the sockets, the latter sustain the arched bars and the weight of the roadway to be laid upon them. But, as the roadway rises in the centre six inches above the sides, another bar of malleable iron, adapted to the curve of the roadway, is stretched above, and from each extremity of the hanging bars to the centre; this bar springs from, and abuts against the cast iron sockets; while the under bar is stretched by the weight of the roadway, and would, in conse-

quence of this, tend to draw the sockets together, this upper bar resists that thrust, and keeps the sockets at their proper distance. It is along this upper bar that the iron-plates for carrying the materials of the roadway are laid and made fast; and, in order to sustain it in the middle, and throw the whole of the weight on the under arch, a series of bearers are raised on the under bar, up to the level of the upper one, and at every 2½ feet in its length. These being properly fastened at top and bottom, serve to unite the upper and under bars, and to form a complete frame, extending from side to side of the bridge, trussed in every part, and capable of withstanding all the weight which can ever be laid upon it.—[Here follow various additional particulars regarding the construction and dimensions of the different parts of the roadway, after which description the author proceeds]:

When all these arrangements are duly carried into effect, we shall obtain a connected mass of strength and stiffness as well as elasticity, sufficient to withstand any pressure or motion to which it will ever be subjected. For supposing a vessel were to strike the bridge, of 200 tons burden, and driving at the rate of six miles an hour, it may be shewn by calculation, that its momentum would not have the effect of pushing forward an equal weight, farther than the space of a foot. But the strength and flexibility of the roadway are greatly superior to this. It would move or bend laterally at least three or four feet without injury; and it would require a weight of at least six times that of the vessel, to produce such a deflection. Besides the roadway, we have the chains, which, if the roadway were yielding, would resist of themselves many times the momentum of the vessel. And, as a farther security, if it were thought necessary, it would be easy to extend between the opposite pillars, and on a line with the roadway, a protecting cable, to check the motion of the vessel before it reached the bridge.

The roadway is connected to the main-chains by the suspending rods. These vary in their length according to their situation in the bridge, being longest at the pillars, and diminishing to nothing at the centre. They are attached below in the manner already described: two rods three-fourths of an inch in diameter being placed under the chains at the distance of 12 inches

from each other on each side of the roadway, and at every five feet in its length. Similar rods are also to be attached between the chains and the roadway on the outside of the pillars, and filling up the space between these and the descending chains. At the top these rods are attached by bolts and eyes to the couplings which bind all the chains together, so that each suspending rod has thus a bearing, not on one, but on the whole mass of chains. But besides the upright rods, I propose, as already stated, and as shewn in the principal drawing, to have cross rods running from the chains to the suspending pillars. The object of these is to keep the arch steadily in its place, and prevent the undulations to which its flexibility subjects it: These cross rods to be extended, if necessary, and, as is shewn in the drawing, quite across the centre of the bridge. They are also to run from the pillars on each side to the descending chains; and, together with the upright rods, will fill up the whole intervening space, while these upright and cross rods, interweaving with each other, will have such a degree of strength and stiffness as to keep the chains here in a straight line, and prevent them from sinking by their own weight, or shaking the pillars by their undulation. These upright and cross rods can be also strengthened and stiffened, if necessary, by diagonals.

In regard to the couplings for binding the chains together, the principal part consists of three bars or prongs standing at right angles to the chains, the middle one passing up between the two middle chains, and the extreme ones embracing the extreme chains on each side; these bars are cast open and unconnected above, but are united below by a cross bar cast in the same piece, and terminating in two sockets, which receive the eyes of the suspending rods, and hold them fast by a bolt. These bars, after being let up through and on each side of the chains, are united at the top by a cross bar fastened to them by nuts or otherwise, and thus the square of chains is completely enclosed. But this coupling is still more securely attached to the chains by bolts passing through the coupling blocks of the chains. At every five feet there are twelve of these coupling blocks, and these are disposed horizontally in two rows, and a bolt passed through all the six blocks of each row, and also through the bars or prongs of the suspending coupling. The

open spaces between the other rows of chains are then filled up, so that this coupling forms a complete bearing on every one of the thirty-six chains which run on each side of the bridge, as seen in the section of roadway. Other forms might be proposed for this coupling, but I think it quite unnecessary to pursue these details any farther, and others of the same kind pertaining to different parts of the structure. In the present state of the business, it is impossible to specify every particular, and it is well known that in cases of this nature, the progress of the work usually points out in the details corrections and improvements which cannot be embraced in the original plan. It is sufficient here to describe the construction in such a manner as to lay down the general principles, which can afterwards be applied in a variety of ways, to be more particularly considered when the work comes to be executed.

On each side of the roadway there is a double rail of 4½ feet high. This double rail consists merely of two single rails, each formed by connecting the lower part of the suspending rods together, or filling up the space between them with smaller rods or wires, interweaving with each other diagonally, as shewn in the drawing. The two single rails being united by diagonal wires or frames, will form together a light, but strong and substantial railing, or fence, on each side of the bridge, and will also greatly add to the stiffness of the roadway. Such, then, is the general construction of the main-chains, the road-way, and the suspending rods or frames; and although I have described them so minutely, I have yet omitted a multitude of details, all which, however, have been duly considered.

The construction of the supporting-pillars is represented in the principal drawing. Each pillar consists of a pyramid with its pedestal, both of iron, and hollow within, resting on a strong pier or pillar of stone, and this founded on a broad platform of wood, supported on a series of piles driven deep into the soil. The pyramid is about four feet square at the top, and eight feet square at the bottom. The pedestal is 10 or 11 feet each way, and the height from the base of the pedestal, which is on a level with the roadway, to the top of the pyramid, is 60 feet. The pyramid and pedestal are formed of iron plates ½ inch thick, and cast with flanches, so as to be put together and bolt-

ed in the firmest manner. The hollow within is built up with stone, and while the weight of the bridge will chiefly rest on the masonry, the iron-casing will both keep the stone together, and will be ready to resist any lateral force which might act on the pillars. The top of the pyramid is surmounted with a strong iron-plate, over which the chains are bent, and on which they rest. The flexibility of the pillar will render the use of rollers unnecessary. The bottom of the pedestal rests on, and is firmly bolted to a strong bottom of iron cast in one piece, and from this bottom there descend a number of strong iron bolts, such as compose the main-chains; these are either made fast to the platform of piles, or, what would be still better, to a strong plate or hoop of iron, embracing the whole compass of the base of the pillar. In this manner a connected chain of iron will be formed, from the base to the top of the compound pillar, and these iron-bolts will also serve to bind the mason-work together. If the pedestal be 11 feet square, it may be formed wholly of stone. The pyramid will then be bolted to a strong cast-iron plate laid on the top of the pedestal, and united to a similar plate on the bottom of the pedestal, by bolts passing through the mason work; this second plate being connected with the third one at the bottom of the pier, by similar bolts passing through the pier from top to bottom; and this last plate being firmly attached to the platform on the top of the piles. To save metal, these plates are cast with four open squares in the middle.

The two pillars, one on each side, support the whole weight of the bridge, and this, in the most extreme case, amounts to about 1000 tons, or about 500 tons to each pillar. But, as the base of the pillar is spread over a surface of upwards of 500 square feet, the pressure on the top of the piles will not amount to one ton on the foot, which is only equal to that of a wall 13 feet high. In fact, the foundation of the present bridge is supporting, and has supported since its erection, a load of three tons on each square foot; so that this puts an end to every idea of the pillar sinking under the load of the bridge. Neither can it be crushed by this load; for, as there are 15 or 16 feet square of stone at the top of the pyramid, where it is weakest, this of itself will be amply sufficient to withstand, without injury, 500 tons,

which is only in that case $\frac{1}{4}$th of a ton or less on the inch, and which even the iron alone would bear with safety.

Lastly, the pillars will be in no danger of oversetting; for as the chains are carried over the top of them, and bent downwards towards the ground, at the same angle at which they hang naturally in the curve, the weight resting on the top of the pillars has no tendency to draw the pillars either to the one side or the other. There will be no strain, therefore, excepting from the weight pressing the pillars directly downwards, and this weight, instead of tending in any degree to overset the pillar, will tend rather, and in a remarkable degree, to keep it steady. So that although the pillar were not fastened at the base at all, it would still require a great force to bend it over, the enormous load at the top keeping it down. It is quite a different thing, as I can easily shew the Commissioners by a simple experiment, from a similar weight of stone or iron resting on the top of the pillars, which would undoubtedly render it top heavy, and liable to be overset by the slightest touch. There the weight acts on the pillars by a pressure or thrust from above; here it acts by a draw from below: the one is the unstable equilibrium which can scarcely at all be preserved, the other is the stable equilibrium which can scarcely at all be overturned; and this is the reason which has made me prefer, as a prop for the chains, a slender pillar of iron in place of stone-work, which, owing to the loose connection of its parts, is less fitted to withstand any undulation in the bridge, and would require, therefore, in order to form a durable support, a much larger mass than what is now proposed, and such as would lead to greater expence.

The foundation of the pillars are to be laid on the site of the present piers, and with their centres within two or three feet of the line of the outside of the roadway, which may be done without taking down much of the present arches. These foundations to be laid at the level of low-water, and on a strong body of piles and planking. By finishing each pier separately, the pillars may be built, the chains suspended, and the whole work go on nearly to a close, without interrupting the usual passage of the bridge. On the northern side of the river these pillars can be built so as rather to strengthen than endanger any farther the present north pier; at the same time I cannot help

recommending to the attention of the Commissioners the re-opening of the Inch channel of the river, as the most effectual method of preserving the foundation of this structure from farther encroachment; from which alteration this good effect would follow, that the violence of the stream, which has carried away the channel, would abate, and the waters in their passage to and from the basin, would naturally deposit new soil, in place of tearing up any farther the bed of the river.

The next object to be considered, is the fixing of the chains in the ground, and this ought to be done so securely, that the chains themselves may sooner be torn to pieces than these fastenings should give way or yield in any sensible degree to the strain. The most effectual way of attaining this object appears to be, to fix at the place where the chains are to be fastened, and which will be about 20 feet under the surface of the ground, as delineated in the principal drawing; to fix here a broad platform of wood, resting on the top of a series of wooden piles, driven deep and firmly into the ground, and strengthened and supported by diagonal piles driven to oppose the strain. This platform to be a square of 20 feet, and so strongly bound together, and to the piles, that it cannot move but in one mass; and after the chains are made fast to it, it is then to be loaded with at least 1000 tons of stone or earth, the part along the chains being built and vaulted above if necessary, that access may be easily had to the fastenings at any future period. The chains, as they approach the platform, are to spread out from their compact square of 15 inches, to cover at equal distances the whole platform of 20 feet, where each is made sure, and furnished, if necessary, with screws to brace it to its proper bearing along with the rest, that each may sustain its due share of the distending force. In this manner the draw of the chains, on each side of the bridge, will be spread over a surface of 400 square feet; even at the utmost stretch, therefore, when the bridge is loaded with 1000 tons, or 500 on each side, there will only be a draw on the platform of $1\frac{1}{4}$ tons per foot; which the weight of the gravel and the stones above will be sufficient to oppose, independent of the effect of the upright and diagonal piles, which would resist, with many times this force, like so many nails driven into a firm board.

In the drawing already referred to, the bridge is represented with the draw-bridge down; but Fig. 4. shews also the situation of the draw-bridge when raised to let a vessel pass. The space which in the principal drawing appears, and on most occasions will really be filled up by the suspending and cross rods, and by the side rails, &c. is, in Fig. 4, when the draw-bridge is up, quite open. All these parts are here formed in two frames or leaves, which, turning on hinges, fold inward from each side to the centre of the bridge, while the roadway part, being made of the slightest and simplest construction, is drawn up by a rope passing over a pulley within each pyramid, and round a barrel within the pedestal, which is turned by a winch applied to the axis when necessary. When the draw is up, the chains being of sufficient height, there is then nothing to prevent vessels passing under them with ease. This part of the work will present no difficulty. But to prevent vessels from striking the roadway while the draw is up, the present wooden pier must be retained and strengthened. For the sake of a more uniform appearance, a similar pier should be retained on the north side, and this might also be turned to a useful account. For if a row of sheet piling were driven along this pier, and projecting on each side with a sweep to landward, the enclosed space might be filled up with sand and gravel, and thus effectually protect the foundation of the present pier of stone. I consider an additional pier of stone for the draw-bridge, on the south side, as wholly unnecessary.

Such, then, is the structure now proposed, in lieu of the present wooden bridge, and if it be executed with care, and with due attention to the principles I have laid down, there can be no doubt of its strength and solidity, while it will form an additional ornament to the scenery of the place, and will be a work altogether worthy of the present advanced state of society and art *.

GEO. BUCHANAN.

Edinburgh, 15*th February* 1823.

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* Since the above Report was written, I last summer visited the great bridge of suspension which is now erecting at Bangor Ferry, over the Straits of Menai, and there obtained much interesting information regarding this

Edinburgh, 18*th February* 1824.

Having, in pursuance of the request of the Commissioners, again considered the subject of the above Report, I see nothing material to alter in any part of the design therein contained. I

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magnificent work. Not feeling myself, however, at liberty to publish all the particulars which were obligingly communicated to me by Mr Rhodes, the superintendant of the iron-work, and by Mr Hazledean, the contractor, I may just state generally, that the span of this arch is 580 feet; depth of arch 50 feet; height of pillars above roadway 53 feet; and height of roadway above the level of high water 100 feet, so that vessels may sail under the bridge. Base of pillars founded on rock near the level of low water, so that, from the base to the top of each suspending pillar, the height is no less than 160 feet. Between these pillars and the high ground, on the opposite shores, the space is made up by three stone arches on the Welsh, and four on the Anglesea side of the strait; and it is only the middle space of 580 feet that is proposed to be spanned by means of the hanging arch. This is to consist of sixteen distinct sets of main-chains, ranged in a manner peculiar to this bridge. Instead of having two compound chains, one on each side of the bridge, and a clear roadway between them, there are here, between the outside chains, two others, of the same dimensions, in the middle; and these middle chains are connected to the roadway by upright suspending rods, in the same manner as the external ones; so that the whole roadway is divided longitudinally into three compartments, the middle one of which, of 4 feet wide, is intended for foot passengers, and the two exterior ones, each 12 feet wide, for carriages, the one for going, the other returning. The object of this arrangement of the chains and rods, is to reduce the distance of the bearing points for carrying the roadway. The whole suspending arch, therefore, consists of four compound chains; and each of these consists of four of the above-mentioned distinct sets of main-chains, ranged one above the other, with about 12 or 15 inches between them. Each set of chains is further composed of six separate chains, formed of square bars of malleable iron, 10 feet long, 3 inches deep, and 1¼ inch thick, worked up to a high state of refinement by repeated heating and hammering. These are swelled out flat at their extremities, and a hole or eye, 3 inches diameter, drilled through each end. These bars are united by short links, the eyes of which are also bored out of the solid, the object of which is to avoid welding the iron, and also by the squareness of the hole, and flatness of the links, to obtain a more equal bearing on the bolts. There is in all the chains together, a section of iron of 330 square inches. The chains are bent over the pillars at the same angle with which they hang on the arch. They take the ground at about 380 feet on each side of each pillar; there they are bent at an angle, and carried, in a sloping direction, 40 feet, into a square tunnel, driven out of the solid rock, and at the bottom of which they are made fast to strong cast-iron plates butting against the rock. The pillars are of mason work, and, like the rest of the arches, are built of a hard and very beautiful limestone rock, which abounds in the neighbourhood. In the bridge it has somewhat the look of granite, and gives the

have merely, therefore, embodied the additional particulars, which have suggested themselves as corrections in that Report. The only alteration which deserves mention, is in the construction of the frame-work of the road-way, which I now propose to form almost wholly of malleable iron, on account of the low price of this material, and the improvements which have been introduced in the rolling of it. This construction will reduce very considerably the weight of metal, while the frame-work will be equally strong, and much less liable to breakage. In regard to the principles on which the above Report is founded, I have taken the liberty, for the satisfaction of the Commissioners, to consult Professor Leslie, and have received from him the following opinion, approving of the plan, and confirming all the opinions I have been able to form on the subject, by his great authority. Having now also received more accurate information respecting the mason-work, and other particulars of expence, I annex a corrected estimate, with confidence that the total sum there stated will cover all the expences proposed. In proof of which, I beg to submit the following letter, which I have received from a very respectable practical engineer, Mr Neilson, of Glasgow.

G. B.

" *College of Edinburgh, 17th February* 1824.

" I have read over with very great satisfaction Mr Buchanan's able Report: it is clear, full, and scientific, yet combining sound maxims of experience with the principles of accurate theory. In every essential particular, it appears eminently distinguished above ordinary reports.

" My approbation is the more hearty and unqualified, since I find that Mr Buchanan has adopted my suggestion, to lower the depression of the bridge, and thereby lessen the strain on the chains. By making that depression the eighth, instead of the fourteenth, part of the whole span, as commonly followed, he

whole structure a very magnificent appearance. Above the roadway the suspending pillar is built solid, excepting two arched passages for the entrance and exit of carriages. At the roadway, it is about 38 feet across the bridge, by 28 feet in the length. At the top it is 34 feet across, by 9 feet. The stones are bound with iron dowels running vertically through them, and bedded in Roman cement. The roadway is to be of wood, supported on malleable iron-frames.

has reduced the necessary weight, and consequently the expence, of the iron to less than one-half. But he has carried economy still farther, by sharing the risk of fracture among several small chains, which enables him securely to employ a coarser and cheaper kind of iron.

" While there undoubtedly would be so much saving in the execution of this plan, compared with any other that could pretend to equal strength and durability, I should be glad, I confess, to see some more expence laid out in the ornamental part. Perhaps the pillars could be constructed of Aberdeen granite, a material of such beauty and strength. It would be highly creditable to the spirit of the Gentry near the ridge of the Grampian Mountains, to erect columns which might emulate the Egyptian obelisks. The traveller would pause, and admire the grandeur of the scene.

(Signed) " JOHN LESLIE,
" Professor of Natural Philosophy."

" *Old Basin, Glasgow, January* 26. 1824.

" DEAR SIR,—I duly received your letter relative to your estimate of the Suspension Bridge proposed to be erected at Montrose. The opportunities I have had of examining the several items of expence calculated on in the construction and erection of this bridge, enable me to state, that I am of opinion that these several sums are adequate to the work. I can give you no stronger proof of this than to mention, that, should the price of iron continue as it is, I should contract to furnish all malleable and cast-iron work at the estimated prices, as well as the execution of the bridge, although the workmanship here is perhaps the only portion of expence that cannot be ascertained with great exactness. The masonry and pile-work, I think, are also liberally provided for; but as the expence of this part of the job depends much on local circumstances, I am not able to speak to it so positively. (Signed) " JOHN NEILSON."

Dimensions and Structure of the proposed Bridge at Montrose.

Span of the arch, or length between suspending pillars, 420 feet.

Versed Sine, or depth of the arch, equal here to the height of the pillars above roadway, 60 feet.

Main Chains.—One compound chain on each side of the bridge, consisting of 36 single chains, running parallel to each other, and ranged in a compact square, 6 broad and 6 deep. Each single chain is $1\frac{1}{2}$ inch diameter; each compound chain has thus a thickness of cross section of $63\frac{1}{2}$ square inches, and both together 127 inches. The single chains are formed of bolts 15 feet long, upset and shouldered at each end, and coupled together with cast iron coupling blocks 4 inches diameter; the strength of each bolt and coupling being carefully proved to eight or ten tons on the square inch.

Roadway.—30 feet wide; 28 feet clear of chains and side rails; and 20 feet clear of footpaths, each of which is 4 feet wide. Roadway laid with *gravel* or metal 4 or 6 inches deep; this laid on cast iron-plates $\frac{1}{2}$ inch thick; these supported on *malleable iron* joisting frames, crossing under the bridge at every 5 feet in its length, each $3\frac{1}{2}$ feet deep in the middle, and well trussed in every part: These frames united and strengthened by malleable iron plates, 2 feet deep, running longitudinally the whole length of the bridge standing upright, and by their depth giving stiffness to the roadway.

Suspending Rods.—These are formed of $\frac{3}{4}$th inch bolts, and are placed two together on each side of the bridge, and at every 5 feet in its length, attached to a socket in the suspending frames below, and to a coupling above, which is attached to the chains, and binds them all into one mass. Similar rods run horizontally from the chains to the pillars, and thence to the descending chains: these serve to keep the arch of the chains steadily in its place, and, interweaving with the upright rods, they communicate a remarkable strength and steadiness to the whole structure.

Pillars.—These are four in number, two at each end of the bridge; each consists of a pyramid, 4 feet square at top, 9 feet at bottom, and 50 feet high, resting on a pedestal 11 feet square and 11 feet high, and the bottom of which is level with the roadway of the bridge. This pedestal rests on a pier of stone, 12 feet square at top, and 20 feet square at bottom, built on the site of the present pier: the bottom is on a level with low water-mark, and founded on a strong basis of planking and piles. The pyramid is composed of iron-plates $\frac{1}{2}$ inch thick, cast with flanches, and bolted and rusted together into one mass, the hollow within being built up with stone. The pedestal is either formed in the same manner, or wholly of stone. The base or pier below is built of ashler, and put together in the strongest manner. The pyramid is bolted to an iron plate or frame resting on the top of the pedestal, this to a similar plate below the bottom of the pedestal, and this again to the platform on the top of the piles,—so that one connected chain of iron runs from top to bottom of this compound pillar, and binds the whole into one mass.

Fastenings.—The chains are made fast at their extremities, each to a cast iron bracket, which is bolted to the top of a wooden platform, 20 feet square, and resting and firmly attached to a series of piles, driven deep and firmly into the soil below. The 36 bolts of the main chain, as they approach this platform, are spread out from their compact square of 2 feet to cover the above platform of 20 feet square, and each is then attached to its bracket. The platform is then loaded with sand, or gravel, till the incumbent

weight amounts to at least 500 or 1000 tons on each end of the bridge, this being the greatest weight the bridge itself will have to sustain.

Strength.—The bridge will carry, with perfect safety, 7000 people in addition to its own weight; and with this great load, no part of the chains will be stretched with a greater strain than 8 tons on the square inch, which it is well known common iron will bear with the utmost safety, but which, at any rate, every bar will be proved to before it goes into the bridge.

Expence.—Estimated at £12,648.

ART. VIII.—*Experiments and Observations on Radiant Heat.* By WILLIAM RITCHIE, A. M., Rector of the Academy at Tain.

1. HAVING procured a differential thermometer with large bulbs, blown very thin, and quite transparent, I placed a heated ball exactly in the middle between the bulbs, and observed that the fluid in the stem remained stationary. I then coated the exterior hemisphere of one of the bulbs with lamp-black, and placed the heated body in the same situation as formerly. The instant the ball was placed in its proper position, the fluid in the stem of the blackened bulb began to descend, and the number of degrees which the fluid descended increased as the temperature of the body was raised.

This experiment appears to me completely decisive of the fact, discovered by De Laroche, that invisible caloric permeates thin plates of glass in the same manner as light. For it must either pass through glass in the same manner as light, or be conducted through it as if the glass were an opaque substance. Now, upon the latter supposition, the only effective part of the heat upon the included air, would be that portion of it which was absorbed by the interior hemispheres of the bulbs, and gradually conducted to the included air; and as these hemispheres are equal, the quantity of caloric absorbed by each will also be equal. Hence it is obvious that the fluid in the stem must remain stationary till the heat reach the exterior hemisphere of each of the bulbs. Now, as this would require some time (since glass is a bad conductor of caloric), how are we to account for the instantaneous depression of the fluid in the stem? Again, since lamp-black allows caloric to radiate more copiously than

glass, it is obvious that the heat communicated to the included air will radiate more rapidly from the half-blackened bulb than from the transparent one, and consequently the fluid in the stem ought to rise, which is directly contrary to the result of the experiment.

Having thus shown that this experiment is incompatible with the hypothesis, that caloric passes through glass in the same manner as through an opaque substance, it will now be easy to prove, that a portion of caloric passes freely through the bulbs of the thermometer in the same manner as light. When both bulbs were transparent, the temperature of the included air would be raised, merely by the caloric absorbed by the substance of the glass; the portion which permeated the bulbs having no power to expand the air through which it was transmitted. Now, if the portion of caloric which freely permeated the interior hemisphere of one of the bulbs, be prevented from passing freely through the exterior hemisphere, an increase of temperature would evidently be the result. The coating of lamp-black completely intercepted this portion, which was rendered sensible by the additional temperature which it communicated to the included air.

I was anxious to vary this experiment, by employing very thin plates of glass as screens; but to procure plates of sufficient thinness, and perfectly transparent, appeared at first a very difficult problem. At length it occurred to me to try portions of a large sphere, instead of plane surfaces. I therefore caused some very large globes to be blown extremely thin, with portions of which, used as screens, I performed the following experiments.

2. Place a plate of polished metal, having an aperture about an inch in diameter, at a small distance above the differential thermometer, one of the bulbs of which being directly under the aperture. Blacken one of the surfaces of the plate of glass, by passing it rapidly through the flame of a candle, and place it directly above the aperture in the metallic plate. Place the heated ball directly above the aperture, and observe how far the liquid in the stem descends before it becomes stationary. Remove the coating of lamp-black by a dry brush or feather, and the fluid will instantly begin to descend, and actually sink se-

veral degrees lower than formerly. The same effect takes place when the blackened side of the lamina of glass is turned towards the bulb of the thermometer.

Let us now examine what would be the effect, upon the supposition that caloric passes through glass in the same manner as through an opaque substance. Since a coating of lamp-black absorbs heat more copiously than a vitreous surface, the fluid in the stem ought to sink farthest when the blackened screen was used. Again, as heat radiates more copiously from a surface of lamp-black than from a vitreous surface, the thermometer ought to sink lower with the blackened side of the screen turned towards the bulb, than with the transparent one, which is directly contrary to the result of the experiment. Hence it follows, that a portion of caloric, which freely permeated the transparent lamina of glass, was retained by the coating of lamp-black, and this portion increased, according as the temperature of the ball was raised.

Lest any objection should be made to these experiments, upon the ground that different surfaces were employed, I had recourse to the following experiment, which puts the matter beyond the possibility of doubt.

3. I coated one side of a thin plate of glass with silver-leaf, and then applied another lamina of glass to the coated side, so that I had now an opaque screen with two vitreous surfaces. I placed this compound screen between the heated body and the bulb of the thermometer, and observed how far the fluid descended in the stem before it became stationary. I then removed the screen, and substituted two transparent plates of the same thickness, and observed that the liquid in the stem descended several degrees lower than formerly.

It can be no objection to the accuracy of this experiment, that one of the screens was thicker than the other by the coating of silver-leaf; for the experiment succeeded even when three transparent plates were used, which makes a much thicker screen, and a worse conductor, than the other.

I have been thus particular in establishing the fact, that invisible caloric passes through very thin plates of glass, in the same manner as light does through thick transparent plates of the same substance, because the fact has been called in question

by some eminent philosophers in this country, and by others denied altogether. It is true, as has been proved by De Laroche, that when the glass is thick, and the temperature of the source of heat low, caloric will not pass through glass like light; but be conducted through it in the same manner as through a plate of metal.

4. Having procured plates of glass of different degrees of thickness, I employed them as screens, as in the preceding experiments, and found, that those plates which stopped the whole current of heat when the temperature of the screen was low, allowed a portion of it to pass when the temperature of the body was sufficiently raised, though still invisible in the dark.

5. We are now prepared to examine into the nature of that power by which heat is propelled from the surface of a heated body. It is generally admitted, that the atoms of caloric are attracted by every other substance, while they mutually repel each other. If hot water be poured into a cold vessel, the sides of the vessel, having a greater attraction for caloric than the water has, will gradually conduct a portion of it to the exterior surface. When the first range of calorific atoms has reached the exterior surface of the vessel, those directly behind exerting their repulsive energy, will propel them with a velocity proportional to this power. Hence, if the temperature of the body be raised, the exterior film of caloric will consist of a proportionally greater number of calorific atoms, and consequently the quantity of radiant heat will also be proportionally increased. If the mutual repulsive energy of the calorific atoms, and the attraction between these atoms and those of the heated body, remained constant at every temperature, then would the quantity of radiant heat increase in geometrical progression, agreeably to the law suggested by Sir Isaac Newton. But this is by no means the case. For, by condensing caloric in a given body, the mutual repulsive energy of the atoms is increased, while the attraction of the body for caloric is diminished;—hence the flow of heat from the surface of a body will increase in a much higher ratio than the increase of temperature. This fact, deduced from the theory which we have shortly stated, was observed long ago by Dr Martine and Erxleben, and has been ably investigated by Dulong and Petit.

6. The theory which we have embraced will enable us to explain, in the most satisfactory manner, the striking difference between the quantities of caloric radiated from different surfaces. If all substances possessed the same attraction for caloric, then would the flow of heat from the surfaces of equal bodies, of the same temperature, be proportional to the number of calorific atoms ranged along the surface of each body. But since the attraction of different substances for caloric varies with the nature and density of the body, it follows that the quantity of heat radiated from the surfaces of different bodies will not be simply as their capacities for caloric, or as the specific heat of those bodies. The attraction of any particular substance for caloric will act as an antagonist-force to the idio-repulsive energy of the molecules of caloric, and, consequently, the greater this attraction is, the less will be the heat which radiates from the surface of the body. From a comparison of various experiments which I have performed with regard to the attraction, capacity, and radiating powers of different substances, the following law seems to obtain: "That the quantity of radiant heat from the surface of a body, is directly as its capacity for caloric, and inversely as its attraction." Thus, the metals, having a powerful attraction for caloric, whilst their capacities for that fluid are small, allow only a small quantity of radiant heat to escape; whilst glass, lamp-black, and paper, having a feeble attraction for caloric, with considerable capacities, allow caloric to radiate very copiously from their surfaces.

7. If the condensation of caloric in a given body increase the idio-repulsive force of the molecules (as happens to every other elastic body), then will caloric, emanating from a body at an elevated temperature, move with a greater velocity than from the same body at a lower temperature. Let us examine how far this deduction agrees with experiment. The molecules of caloric, in passing through glass, must suffer a considerable resistance from the attraction of the glass. Now, when the velocity is small, the resistance may be such as completely to stop the whole current of heat. If the velocity, however, be increased, by raising the temperature of the body, then many of the atoms of caloric will overcome the resistance, and find their way directly through the glass; whilst others, impinging against the solid

atoms of the glass, or at a disadvantageous place, will either be reflected or retained by the glass. The reason why caloric, moving with a small velocity, passes through very thin glass, whilst it is completely stopped by a thicker plate, is hence perfectly obvious, the resistance in the one case being much greater than in the other.

This explanation receives additional support from the following curious fact, discovered by De Laroche, that, if invisible caloric pass through several very thin plates of glass, a greater proportion of it is detained by the first plate than by the second, a greater proportion by the second than by the third, &c. According to the theory which we have adopted, it is obvious that some of the molecules of caloric will be thrown off in more advantageous circumstances than others. Some of them will be thrown off by an oblique impulse, whilst others will be darted forward by those acting directly behind them. Besides, some will have their velocities diminished by others crossing their paths, while others will have their velocities increased by favourable collisions. Hence it is probable, that the atoms of caloric, emanating from the same source, do not all move with the same velocity. If this be the case, then it is obvious that all the atoms moving with a comparatively small velocity, will be detained by the first plate, and only those allowed to pass which move with a velocity sufficient to overcome the resistance. Now, all those which escape the grasp of the first plate, will have a velocity sufficient to permeate the second, and consequently a much smaller proportion will be detained by it.

8. If there be any truth in the above theory, then it is obvious that radiant heat moves with every possible velocity, till it verge into that of light. Having arrived at such a velocity, it will then pass through the various humours of the eye, and produce an impression upon the retina. Hence it is probable that light is merely caloric moving with an increased velocity.

(To be continued.)

Art. IX.—*Remarks illustrative of the Influence of Society on the Distribution of British Animals.* By the Rev. John Fleming, D. D. F. R. S. E. M. W. S. &c.

It is customary with antiquaries in general, to delight to dwell on scenes which exhibit to their imagination the memorials of events nearly forgotten, or the transactions or customs of distant ages. Events of a more recent kind, or transactions which may be considered only in progress, do not arrest their attention, or at least fail to excite in their minds that deep interest which remoter subjects readily awaken. The public likewise feel and act much in the same manner. There is an importance attached to remote events, depending solely on their antiquity; while there is a vulgarity inseparable from recent events, founded on the supposition of their being well known.

Geologists have long acted, and we fear still act, in a similar manner to antiquaries. The study of the characters of the older strata (their position, structure, and ingredients), has been pursued with greater ardour than any researches which have been entered upon with the view of illustrating the connections of the newer deposits. Coal, sandstone and limestone, have been examined with zeal, while few have bestowed their attention on peat, sand, or marl. Similar practices have prevailed with regard to organic remains. Many have speculated concerning the structure and habits of those relics which occur in the solid strata, who have not deemed the study of the laws which regulate the living races an object of much consequence.

We are not disposed to refer these well known habits of the antiquary and the geologist to any natural preference of the obscure to the distinct,—of subjects, the nature of which may be illustrated with certainty, to those, the relations of which, circumstances have rendered us incapable of determining. We are aware that antiquarian and geological speculations are usually engaged in from motives of amusement; and, when conducted in the usual manner, seldom fail to gratify their votaries. Were they to commence their investigations, with a knowledge of recent events, and proceed by degrees to those of remoter

times, their conclusions would assume a more imposing character, but the accompanying labour would be greatly increased. In speculating on the affinities of recent events, the understanding is ever occupied with facts; and the imagination, thus in trammels, dare not indulge in its licentious wanderings. But when remote events are the subjects of our contemplation, fancy becomes a valuable assistant, by reuniting disjointed links, supplying that which is wanting, and enabling the mind to arrive at conclusions, which, without the labour of investigation, had probably been long anticipated.

In Britain, there are many individuals, possessed of much zeal and sagacity, who have long been directing their attention to those organic remains which occur in the different regularly stratified rocks, and many important facts in their history have been satisfactorily established. More recently, public attention has been excited, in a remarkable degree, by the accounts which have been communicated of those singular remains of quadrupeds and birds found in many limestone caves, and loose depositions, which have been termed *diluvial* by several English mineralogists. These quadrupeds are supposed to have been destroyed by the last grand catastrophe that our planet has experienced, and which, in the opinion of many, is identical with the deluge of Noah. At this point, the researches of the geologist have terminated; and the truly interesting interval succeeding that period, and extending to the present time, has been neglected as *too recent* for inquiry or speculation. To us, however, it appears under a different aspect; and we trust that the observations which follow, will, to a certain extent, justify the propriety of our views, and even excite others, who are placed in more favourable circumstances, to contribute to the illustration of a subject, calculated to throw much light on the more ancient revolutions of our globe.

Man, whether we view him as a savage or a citizen, is induced, by various motives, to carry on a destructive warfare against many animals, which he finds to be his fellow residents on the globe. If we consider him as supporting his savage character, we witness the chase of the *graminivorous* animals, entered upon as a measure of necessity to supply the cravings of his *appetite*. He observes their habits, their haunts, and their migra-

tions,—the hours at which they feed, the periods of their repose, and the season of the year at which they bring forth their young. Guided by his experience, he commences his attack with such effect, that the resources of the antagonist are generally insufficient to provide a defence, or secure a retreat. But he has other wants to provide for besides those connected with his supply of food. The skins of animals, with but little preparation, afford him comfortable *clothing*, a covering to his tent, or a water-proof casement for his boat; while their fat supplies fuel for his lamp, or an ointment for his skin.

The savage does not, however, confine himself in his hunting parties to the graminivorous animals. The *carnivorous* kinds are equally the objects of his anxious pursuit. Many of these dare to wage war with himself,—to dispute a track through the forest, or mountain gorge, or fearlessly and successfully rush into personal combat. The extermination of such opponents is pursued as a measure of *safety*. But the animals that thus readily make their prey of the savage himself, in company with many others, less powerful, pursue the graminivorous kinds, divide the prize with him, or bear it away. The savage thus carries on a war against the graminivorous animals, chiefly to procure food and raiment; while he no less zealously endeavours to destroy the carnivorous kinds, as endangering his personal safety, or interfering with the objects of his pursuit.

When man emerges from his rudest, or merely hunting state, to the *pastoral* condition of society, his attacks on the graminivorous animals are not less keenly or successfully conducted; while those which he has reclaimed by his sagacity, require a great share of his attention. These have their natural provisions against danger greatly reduced by domestication; and consequently require for their safety the protection of their owners. To secure this safety, man is led to carry on a more vigorous war against all those carnivorous kinds which he formerly persecuted, and perhaps others which he now finds disturb or kill the young of his herds. If he rises from the pastoral to the *agricultural* state of society, he finds that the enemies of his fields are equally numerous with those of his flocks; that the catalogue of his foes has become greatly extended; and that he

must assume the character, not merely of the master, but the tyrant of the soil.

The war which man thus carries on against the lower animals, is influenced, in a remarkable degree, by the progress of society. The wants of man increase in kind and variety with his advances in civilisation, and his means of supplying them become proportionally numerous. The war carried on by rude tribes is limited in its objects, and uncertain in its results. But with the progress of experience and improvement, the objects of the chace cease to be limited, while the methods of capture, and engines of death, become more numerous, complicated, and effectual. The war is likewise influenced by circumstances of a local nature. In the warmer regions of the earth, the physical obstacles against which animals have to contend, are not so numerous as in the temperate and colder regions. There, too, man is not so intelligent or energetic. But in the higher latitudes, the changes of the season exercise a more powerful influence, and, at intervals, place animals within the reach of capture, which at other times are in comparative safety. Here, too, man, having a more scanty supply yielded him by the vegetable kingdom, is compelled to be more active in the chace, and the climate in which he resides fits him, in a peculiar manner, for hardy enterprize.

In attending to the checks imposed on the increase of many of the lower animals, we must view them, not merely as the objects of the continued persecution of the human race, but as equally exposed with man himself, to experience the depopulating influence of various physical changes,—the volcanic eruption or earthquake,—floods,—droughts,—frosts or snows,—and epizooties. Severity of season and contagious disease, may account indeed for the accumulation at one spot of more individuals of a species, or of different species, than the quantity of food produced by the district could be supposed capable of maintaining.

If we consider the dispersion of the human race over the earth's surface, (for to what spot has man failed to find his way?), and the unremitting persecution which they have carried on against the lower animals, during the long term of nearly 6000 years, varying their destructive weapons with the pro-

gress of improvements, and extending their ravages with the increase of their wants, we shall arrive at the conclusion that man must have altered greatly the geographical range of many species, and may even have succeeded in effecting the total destruction of a few. But we are not left to conjecture on this interesting subject. Though the preceding observations have been considered necessary to remove prejudice, and prepare the mind for examining the *proofs* which are about to be offered, they are only to be viewed as establishing the probabilities of the question. Numerous facts seem to offer themselves in illustration of the subject. Those connected with BIRDS shall be first attended to.

The improvements which have taken place in agriculture, within the recollection of the oldest inhabitants of the country, and the extended use of fire-arms, have produced such remarkable changes on the haunts, the shelter, and the means of subsistence of many birds, as to banish from extensive districts several species which were formerly abundant. Eagles, Ravens, and Bustards, have entirely disappeared from the more cultivated districts. The haunts of the Mallard, the Snipe, the Redshank, and the Bittern, have been drained, equally with the summer dwellings of the Lapwing and the Curlieu. But there yet remain districts to which these species can resort, and continue to maintain their claim to be regarded as British subjects, though the limits of their residence have been greatly reduced. Others, however, have shared a different fate.

The Capercailzie or Wood Grouse, formerly a native of the pine forests of Ireland and Scotland, has been destroyed within the last fifty years. It still, however, maintains its station in the more extensive woods of Northern Europe and Asia. Those which survive in the Norwegian forests, are likely soon to share the same fate as the ancient inhabitants of our own woods, as numbers now find their way, during the winter season, to the shops of the London poulterers *. Attempts, it is understood, have been recently made to restock our pine forests in the north with this noble bird from their continental haunts.

* Some hundreds of these birds are annually sold in the market of Christiania in Norway.—EDIT.

The Egret, which does not at present occur but as an occasional visitant, appears to have been plentiful about the middle of the 15th century. A thousand individuals are stated to have been used at the celebrated feast of Archbishop Nevill's in the reign of Edward the Fourth *.

The Crane, now equally rare with the Egret, appears, even so late as the days of Willoughby, to have occurred in abundance. In his "*Ornithologia*," he says: "sæpissime ad nos commeant, suntque in palustribus agri Lincolniensis et Cantabrigiensis, æstivo tempore magni eorum greges," p. 200. Ray, when repeating the same observation, states their visits, (probably with greater propriety) to have been "hyberno tempore," *Syn. Av.* p. 95. That they were equally common in Scotland a century previous to the observations of Willoughby, appears evident from the following passage of Lesley, "*De Origine, Moribus et Rebus gestis Scotorum*," Rome, 1678, p. 25. "Cæterum quia nihil est ex omni parte beatum, quod una parte nobis redundat, alia est diminutum; nam ciconiæ, fasiani, turtures, turdi, philomelæ, apud alias nationes frequentes, vix apud nos inveniuntur: *grues plurimi*, sicut et ardeæ: olores autem, quorum apud Anglos magnus est proventus pauciores."

Though these birds are now expelled from Britain, they still continue on the Continent of Europe and Asia, to maintain their existence, favoured by the great extent of waste ground, and the comparatively slow progress of improvement in society.

It may now be asked, If these birds have so very recently suffered expulsion from the British Isles, how many more species must have been extirpated since the Celt first took possession of the country, or even since the Roman armies were withdrawn? But on this subject we presume not to speculate, as we have neither documents left us in history, nor memorials in nature.

The influence of the progress of society on the species of British *Quadrupeds* will appear to have been equally extensive.

* There are some circumstances connected with the known habits of the birds stated to have been served up at this feast, calculated to shake our confidence in the truth of its history, or at least in the accuracy of its historian. Woodcocks and cranes, as well as ruffs and quaills, appear in equal abundance, though the former visit the country only in the winter, the latter in the summer.

As furnishing articles of subsistence or luxury for the table, the efforts of the chace have been long directed against the three species of *deer*, with which the country formerly abounded. In the days of Lesley, the pursuit of the stag, fallow-deer, and roe (" cervum, damam aut capream"), by means of blood-hounds and greyhounds, constituted the noblest sport. And when he informs us, that from 500 to 1000 were sometimes slain at a hunting-match, we cease to be surprised at the diminution of the breed. Were it not for preserved forests, indeed, the native races would soon be extinguished.

The pursuit of several of our native animals, for the sake of their *fur*, has long been a favourite object with the hunter, and the articles of export which it has furnished have been deemed worthy of legislative enactments. The Otter, the Martin, and the Polecat, have in consequence been reduced within narrow bounds.

Noxious quadrupeds, or such as are injurious to the poultry-yard, or the fold, as the Wild-Cat and Fox, have in like manner, in recent times, been greatly reduced in numbers. About the middle of last century, several fox-hunters, (by trade), were to be met with, each keeping a small pack of well-trained hounds, frequenting, periodically, different districts of the country, sojourning for a few days at the house of a farmer, where having destroyed the *vermin* of the neighbourhood, and received a small recompence in money, they departed to another quarter, where their services were needed and expected. But the hireling fox-hunter is not now a trade which the state of the country requires*. His employment has passed into the hands of gentlemen, and ceasing to be viewed as mean or vulgar, is entered upon with zeal, as an agreeable and athletic exercise. But, in some places, the objects of the sport have become so scarce, that it is necessary to import the victims from the yet half-reclaimed districts.

Badgers appear, likewise, at a former period, to have been more numerous than at present. There are several places, the appellations of which have the prefix Brock or Badger, at-

* **Fox-hunters are still employed in Morven, Isle of Skye, and other Highland districts.—Edit.**

testing in the Gothic name the prevalence of these animals, and the Scandinavian power by which it was imposed.

The preceding quadrupeds have been merely reduced in numbers, and expelled from certain districts; others, however, have proved too feeble to support the war carried on against them, and do not at present exist in a wild state in the country. So early as the period of the Roman Invasion, the Britons appear to have paid great attention to the *Horse*, as a useful engine of war, as is attested by Cæsar, in the description he gives of their cavalry, (*Cæs. Com.* lib. iv. 33.) The excellence of the breed, even in the reign of Athelstan, may be inferred, from his prohibitory statute, preventing their exportation. At what period the wild breed became extinct, we have not the means of determining. If we are to credit Hector Boece, troops "equorum indomitorum" frequented Inverness-shire so late as the end of the 15th century.

Of the ancient indigenous *Oxen* of this country, two different breeds appear to have existed. The "*vaccæ non cicures*" of Lesley, (p. 10.) which frequented the mountainous regions of Argyle and Ross, were probably the parent stock of the present breeds of cattle, which yet preserve a considerable uniformity of character, in the more remote districts of the country. The "*boves sylvestres*" of the same author, remarkable for their white colour, with the muzzle and ears black, were, even in the days of Sir Robert Sibbald, (*Scot. Ill.* p. 7.) to be met with in many of the mountainous regions of Scotland. The remains, indeed, of this breed are yet preserved in the parks of a few of our nobility. Our ancient historians give to the last varieties *manes*, confounding them probably with the aurochs of Germany (Bos Urus), a species, the claims of which to a place even in our extinct Fauna is doubtful.

The *Wild Boar*, once a native, abounded in the reign of Henry the Second, in the great forest then existing to the north of London, according to the testimony of Fitzstephen. In Scotland, the parish and family of Swinton owe their name to this animal, the former celebrated for harbouring, the latter for destroying them. The period of their extirpation is unknown.

The fur of the *Beaver* has ever been highly prized, and eagerly sought after. At the close of the 9th century, this

animal had become scarce, and, by the 12th century, was only to be met with, according to Giraldus de Barri, in one river in Wales, and another in Scotland. If we may credit Boece, they were found in plenty, even so late as the 15th century. It was termed Llosdlydan by the Welch, in the 9th century, and, in the Highlands of Scotland its Erse name, Losleathan, still holds a place in the vocabulary of the Gael.

Among the noxious animals which have been extirpated by the efforts of the hunter, we may enumerate the Wolf and the Bear. The Wolf continued in Ireland to maintain its ground, so late as the beginning of the 18th century (1710), though it had been extirpated in Scotland thirty years before, and in England at a much earlier period. The *Bear*, which in Wales was regarded as a beast of the chace, equal to the hare or the boar (*Ray. Syn. Quad.* p. 214.), only perished, as a native of Scotland, in the year 1057.

To this extensive catalogue we might have added another,—the Antelope, the remains of which, along with the bones of deer, boars, and beavers, were found in a peat-bog at Newbury, Berkshire.—*Phil. Tran.* 1757, p. 112. But there is no accompanying description, indicating to which of the two European species (A. Rupicapra or Saiga) it belonged.

From the preceding statements, we seem warranted to conclude, that the progress of society is exerting, and has exerted a powerful influence on the geographical distribution of British animals. A few species, which formerly roamed through the woods, in freedom, have been reclaimed from a savage state, as the horse, the ox, and the boar. Others have been so persecuted by the sportsman, or disturbed by the farmer, that they have been compelled to retire to the more uncultivated and inaccessible districts, as a temporary asylum from their foes. While a third class, furnished with fewer resources, and exposed to a severe persecution, have perished from off the land.

The same effects appear to result from the progress of society throughout Europe, and, we may add, throughout the whole earth. The animals, which in this country have been driven to a corner, are, on the Continent, experiencing similar encroachments on their haunts. The animals which society, in this country, has succeeded in extirpating, have, in like manner, been

banished from many districts on the Continents, and are daily becoming scarcer in those places where they yet maintain themselves.

These changes have all taken place in the course of the last six or eight centuries. In the ages which have preceded, the same causes must have been in more or less active operation, and the chasms produced in our Fauna, in consequence, may have been much more extensive, than, without due consideration, we are disposed to admit. But on this subject, the documents of history are unavailing, and the voice of tradition is silent. Nor need this excite our astonishment. The objects now interesting to us, from their scarcity, must have been uninteresting to the early inhabitants of this country, from an opposite cause. And the changes which we are so eager to investigate, would, from their being gradual, fail to arrest the attention of savage tribes. Fortunately, however, the silence of history, though sufficiently annoying, need not prevent us from conducting our researches into the changes of remoter times. Memorials remain, as unequivocal as those of history, attesting the revolutions which certain species of animals have experienced, once the cotemporaries of man, and the lower animals still his companions on the earth.

In our *Peat-Bogs*, we find the remains of those ancient forests, with which the country was clothed, even to a comparatively recent period, and in the marl beds which occur below, are preserved the memorials of those various animals to which these woods afforded shelter and nourishment. In these marl beds, the recent formation of which is not disputed by any class of geologists, are occasionally found the bones of the horse, ox, deer, boar, and beaver, in company with the shells of those moluscous animals which are yet to be met with in almost every pool. Among these remains, some of the bones seem to have belonged to larger individuals than are to be met with among those varieties of the species which are known at present. This remark is particularly applicable to the skulls of the ox, and the horns of the stag, which frequently occur.

Were these skeletons, the only memorials of the former inhabitants of the country, they would add nothing to the documents furnished by history. Their accompaniments, however, are of the

most interesting kind. If ever other quadrupeds existed, and were extirpated by the chase, we may expect to find their relics in those very beds which have preserved to us the remains of such as history intimates to have been once indigenous. This circumstance would constitute the connecting link between the known and the unknown,—the fabulous and heroic ages—the transition from the flint-pointed javelin and stone-hatchet, to the sword, the spear, and the musket.

In the marl bogs of Ireland and England, as well as of several places on the Continent of Europe, the remains of a species of *Elk* are found occasionally. This species is not known at present, in any country, in a recent state. It resembles somewhat the American elk, but seems possessed of characters intimating a specific difference *.

A specimen of the horn of the fossil *Rhinoceros*, found in one of the marl pits at the loch of Forfar, (*Wern. Mem.* vol. iv. p. 582.) exists at present in the Edinburgh Museum; and we have been informed by Professor Jameson, that two other examples have occurred in Blair-Drummond moss, on the banks of the Forth. Is is to be hoped that the skulls will yet be procured.

A Hippopotamus is likewise recorded by Lee, in his Natural History of Lancashire, as having been found under a peat-bog in that county.

These animals, formerly inhabitants of this country, have their remains preserved, not only in peat-bogs and marl beds, (deposits which, from the commencement of their formation to the present time, have experienced no remarkable geological change, and indicate the absence of any physical revolution which could occasion the death of the individuals now represented by these bones), but likewise in the *silt* of our great rivers. In the valley of the Thames, for example, they occur in the regular stratified clay, sand, gravel, and peat. In Mr Trimmer's account of these remains, as they occur at Brentford, he enumerates the following animals, (*Phil. Trans.* 1813, p. 133.):

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* A nearly complete and splendid specimen of the skeleton of this species is preserved in the College Museum of Edinburgh, of which a figure and description will be given in our next Number.—Edit.

hippopotamus in great plenty; two kinds of deer; two kinds of elephants; the ox; together with land and fresh-water shells.

In the loose strata of sand, gravel, and clay, the remains of quadrupeds, such as have already been mentioned, occur in abundance. The Irish elk has been found in such circumstances at Walton, in Essex. In many districts of England, the remains of the mammoth, rhinoceros, horse, ox, deer, hippopotamus, and hyæna have been detected. In Scotland, two examples of the elephant have occurred; the first was found at Greenhill sandstone quarry, near the Water of Carmel, in the parish of Kilmaurs, Ayrshire, in January 1817. Two tusks and some small bones were found 17½ feet below the surface, imbedded in clay *. The substance of the bed, at a distance from the remains, was of a light brown colour, but in its immediate neighbourhood it was of a dark brown colour, and emitted a most offensive smell, (*Mem. Wern. Soc.* vol. iv. p. 64.). It has been stated, that several *marine* shells were found along with these relics, in the dark coloured earth; but the names of the species have not been mentioned, nor any specimens produced to justify the assertion. The second instance occurred to the workmen in digging the Union Canal. At the west park of the estate of Cliftonhall, in the county of Edinburgh, on the 18th of July 1820, a large tusk was found in a thick bed of clay, 15 or 20 feet below the surface, (*Ib.* p. 60.) In the same kind of clay or *till*, as it is provincially called, and at no great distance, viz. on the estate of Bonnington, Edinburghshire, the workmen, in excavating the canal, found a copper battle-axe, (*Edin. Phil. Journ.* vol. vi. p. 357.) It was imbedded four feet deep in the bed of clay, which was covered with seven feet of sand and nine of moss †.

The strata in which these relics have occurred, are termed by Mr Bald "Old alluvial cover," and by many English mineralogist "Diluvium," in the belief that it was formed at the universal deluge. The proofs, however, of the propriety of the latter term, and of the hypothesis with which it is connect-

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* A portion of one of the tusks is preserved in the Edinburgh Museum; the other is in the possession of the Earl of Eglinton.—Edit.

† We question the accuracy of the statement referred to in the text.—Edit.

ed, have ever appeared to me extremely faulty. The partial occurrence of these strata, their limited extent, great difference of character in neighbouring districts, the presence of the remains of terrestrial animals, and the absence of marine exuviæ, demonstrate that a "*universal*" flood, possessing the velocity which some have assigned to it, had no share in this formation. The phenomena which they exhibit, indicate a cause, partial, sudden, and transient, like the bursting of a lake. It is true, that many mineralogists assign to the waters of Noah's flood, a progressive motion in one direction (differing in character from tides), sufficient to tear in pieces beds of granite, excavate deep valleys, and deposite the spoils of the rocks of Labrador or Norway on the plains of England. They, however, admit, that it so far respected British productions, as neither to have floated into the Atlantic the quadrupeds which it drowned, nor the boulders of chalk which it produced, but permitted them to remain in the neighbourhood of the place of their birth. The direction of this impetuous current, so widely different in character from that which Moses assigns to the flood, is imagined by some to have been from the north, or, by others, from the west, or from the south, just as their preconceived notions dictated, and in the absence of all respect for the valuable Linnean rule, "The genus should furnish the character, not the character the genus *."

The relics of the animals which we have now been considering, are not confined to beds of marl or clay, but occur in fissures and caves in rocks. In the celebrated Cave of Kirkdale in Yorkshire, the bones of a species of hyæna, of tiger, bear, wolf, fox, weasel, elephant, rhinoceros, hippopotamus, and horse, two species of oxen, three species of deer, a species of hare, rabbit, water-rat, mouse, raven, pigeon, lark, duck, and snipe, have been identified. These relics were imbedded in soft mud, and covered with calcareous stalagmites. The occurrence of the bones of hyænas in great profusion in this cave, their excrement, the marks of their teeth on the other

* It has been said that Werner advocated the *geological diluvian hypothesis*. This we know was not the case. On the contrary, his opinion was nearly the same as that stated in the text by Dr Fleming. With equal accuracy it has been said that Mohs was of the same opinion.—Edit.

bones, left in small angular fragments, have led Professor Buckland, in his interesting work, "*Reliquiæ Diluvianæ*," to conclude, that the cave had long been occupied as a den by this animal, which had carried into it the remains of its prey. The valuable notices which Dr Knox gives (*Wern. Mem.* vol. iv. p. 385.) of the habits of the Cape hyæna, in which he states, that that species never carries off the bones or carcases it preys on, into dens, have been supposed to be hostile to the conclusion to which we have referred. The objection would be formidable, if it applied to the *individuals of a species*, but is without force in reference to the *species of a genus*. Our knowledge of the habits of the hare would not qualify us for speculating safely concerning those of the rabbit, and the same remark applies to the raven and rook,—the swallow and sand-martin. Each species possesses peculiar habits, and is influenced in its physical distribution by peculiar laws. The Kirkdale hyæna is a different species from those of Africa, and is now extinct. It may have had different habits; its physical distribution has been different. The dispersion of its remains throughout Europe, proves the latter; the appearances of the Kirkdale Cave, intimate the former *.

In the Cave of Hutton, in the Mendip Hills, connected with an open fissure, the remains of the elephant, horse, oxen, two species of deer, skeleton of a fox nearly complete, large bear,

* Mr Goldfuss, in an article on the fossil lion and tiger, adduces a remarkable passage of a German poem of the thirteenth century, entitled *Neibelungen*, in which a great hunt is described, where, and independently of the *elk*, the *bison*, the *urus*, the *stag*, the *boar*, the *bear*, mention is made of a *lion*, and of two animals, of which the one is named *schelch*, and the other the *halbwolf*, a term which signifies *half-wolf*. The schelch occurring in the same strophe with the elk, the bison, the urus, and the stag, all of them large ruminating animals, M. Goldfuss supposes the poet had in view the great elk (*Cervus giganteus*), which he thinks might also have been the animal of the Hercynian Forest indicated by Cæsar (Bell. Gall. lib. vi. c. xxvi.) under the name of *bull, with the figure of a stag, of which the male and female carried on the forehead a single horn, divided into branches like a hand.* With regard to the *halbwolf*, this naturalist is of opinion that the hyæna is meant by it, not that he believes that at the time of the *Niebelungen* these animals still existed in Germany, but he supposes that the poet had learnt its former existence from tradition. This statement we consider to be interesting as tending to support the view given by Dr Fleming in his able memoir.—Edit.

and hog, have been discovered. In the caves at Plymouth, the skeleton and bones of all the animals already mentioned, have occurred, and in such circumstances as to indicate that they had been washed out of an open fissure by some land-flood, and deposited in confusion in the neighbouring caverns.

Professor Buckland, in his speculations concerning the antiquity of those bones found in fissures, and caverns connected with them, conceives them to have been drifted into their present position by the waters of Noah's flood; that the mud by which they are surrounded was a deposite from these waters; and that the stalagmite, covering the whole, is post-diluvian or recent. Several obvious circumstances render such an opinion untenable, among which one may be noticed, which occurred to the author himself. In the cave of Wokely Hole near Wells, at the south-west base of the Mendip Hills, bones and teeth of human subjects have been found, together with a fragment of a sepulchral urn. The mud was here present, as well as the stalagmite, but the mud is declared to be "evidently fluviatile and not diluvian," and the bones "are very old, but not antediluvian." It appears that a subterranean river runs through the cave, which may have deposited the mud during its highest floods. But why may not the mud in the Kirkdale Cave have been deposited by a similar agent? According to the statements of the Rev. Geo. Young (*Wern. Mem.* iv. p. 264.), "it is not a mere fissure in the rock, as is evident from the want of correspondence between the opposite sides, and from the existence of a number of rounded hollows or depressions, appearing in the sides, the floor, and even the roof; resembling such water-worn hollows as we see in rocks, in the beds of rivers, or on the shores of the ocean."

In a cave, likewise, in the Mendip Hills, at Compton Bishop, (*Reliquiæ Diluvianæ*, p. 166.), numerous bones of foxes were found, and fifteen skulls extracted. In an open fissure at Duncombe Park, (*Ib.* p. 54.), the bones of dogs, sheep, deer, goats, and hogs have likewise been found lodged.

In addition to these facts, it may be added, that in the fissure of the gypsum at Köetritz, in Germany, described by Baron Schlotheim, a translation of whose paper on the subject has appeared in the *Annals of Philosophy*, vol. xxi. p. 17., the remains

of *man*, and several recent European quadrupeds, and even the domestic cock, occur along with the remains of the extinct rhinoceros, hyæna, tiger, &c. "In Winter's gypsum quarry, human bones were discovered, at the depth of 26 feet from the surface, lying 8 feet below the bones of the rhinoceros, there also deposited *."

In the preceding statements, we witness British animals, in reference to the changes to which they are subject, capable of being arranged into three classes,—the first including those species, the individuals of which are daily becoming scarcer, in consequence of the agency of man,—the second, those which man has succeeded in extirpating, but which still find an asylum in the more thinly peopled or less cultivated districts of Europe,—while the third embraces those, which, though once natives of Britain and Europe, have ceased to exist in a living state on the earth. As connected with Britain, we may enumerate in the last class the elephant, rhinoceros, hippopotamus, hyæna, cave-bear, tyger, and elk. To what cause, then, are we to assign the extinction of these animals: To the extirpating effects of civilization, or to physical revolutions, over which man had no controul?

In order to solve this important problem, it is necessary to keep steadily in view the following conditions, which have been determined:

1. The remains of these extinct animals occur in company with those which have been destroyed or extirpated by human agency, and with such as still survive, and seem suited to the climate. Thus, the bones of the extinct elephant, rhinoceros, and cave-bear, occur along with those of the common bear, the wolf, the fox, and the horse.

2. The remains of these extinct animals are not found in any situation, such as caves, fissures, gravel and clay-beds, peat or

* The human bones found at Köstritz, according to later accounts, are said to occur, not in the regular alluvial deposite containing remains of elephants, &c., but in an irregular and accidental deposite of a comparatively modern date. This being the case, we do not yet possess any authentic instance of human remains occurring in those beds that contain bones of elephants, rhinoceroses, &c.—Edit.

marl, in which the relics of existing animals may not be placed, and have not been found.

3. The remains of extinct animals occur in situations, indicating the action of causes at the period of their burial, not now frequently in operation, as in thick beds of clay or gravel; and they likewise are found under circumstances, which prove, that no remarkable physical change was taking place at the time, nor has taken place at any subsequent period—as in beds of peat and marl. The same remark is equally applicable to the remains of recent and extirpated animals.

4. The extinct species have had a geographical distribution in Europe, similar to the recent and extirpated kinds with which their remains are intermingled. In considering the geographical distribution of animals preparatory to the investigation of their physical distribution, we should ever bear in mind the great changes man may have occasioned in the former. This will induce us to attach suitable importance to their fossil relics, as the indication of the extent of their former dispersion. But, when we find a *great* difference prevailing in physical distribution, between two animals referred to the same species, as the Siberian rhinoceros, and the recent African one, the skull of which was brought home by Mr Campbell, we should not infer identity but from close and extensive resemblances of structure.

5. The remains of these extinct animals occur only in the superficial strata, and in fresh-water gravel or clay, and may be viewed as connected with the last or modern epoch of the earth's history.

6. Man was an inhabitant of this country at the time these animals, now extinct, flourished, his bones and his instruments having been found in similar situations with their remains.

The natural history of these remains of extinct animals, demonstrate that opinion to be erroneous, which considers the deluge as having drowned these animals, while it formed the beds of gravel, clay and loam, in which they are now imbedded. If it drowned the hippopotamus, how did the ox and the horse escape? Why this partial selection of its victims among the ancient inhabitants of the country? Professor Buckland, who has defended the opinion which ascribes the extinction of these

animals to the deluge, classifies the alluvial strata into such as are diluvian and such as are post-diluvian. But these organic remains occur, not only in the so-called *diluvian clay*, but in the acknowledged *post-diluvian marl*; and this flood, instead of consigning indiscriminately to a watery grave, all the quadrupeds of Britain, selected as the objects of its destruction, only such as in all ages must have been most eagerly sought after by the huntsman, and such as his efforts would, long before this period, have annihilated.

The whole circumstances of the case lead apparently to the conclusion, that the weapons of the huntsman completed the extinction of these animals, from the first ages the object of his persecution; though we can feel no hesitation in admitting, that murrains, severe seasons, and local inundations, may have accelerated their ruin *. The destructive influence of these circumstances, must, indeed, in all ages, have operated in checking the extension of particular species; nor has man himself been exempted from their ravages. But as his resources multiply with the progress of society, while those animals against which he contends, become more exposed to his attacks, man has outlived these changes, along with those brute cotemporaries which have not been the special object of his persecution.

The preceding remarks, offered on a very interesting department of the natural history of the earth, may serve to point out the rashness of those attempts which have been made to unite the speculations of geologists with the truths of Revelation. Without controversy, the works and the words of God must give consistent indications of his government, provided they be interpreted truly. The talent, sagacity, learning, and industry, occupied, for ages, with the Book of Revelation, have produced a mass of evidence, by which its *moral authority* has been esta-

* Professor Link, without attempting any explanation of the manner in which the quadrupeds, whose remains are found in alluvial strata, have disappeared from off the face of the earth, maintains that we have no geological evidence for their destruction by a great flood, for he remarks, "We find remains of these extinct species in the same beds as those enclosing bones of living species; therefore, if the one set were destroyed by a universal deluge, the others must have suffered at the same period.—Edit.

blished. But, unfortunately for the interpreters of the Book of Nature, they have been few in number, their field of observation too limited, and their prejudices too obvious, to permit any high value to be attached to their theoretical deductions; as the history of mineralogical science in Britain for the last twelve years abundantly testifies. It would be favourable to the progress of geology, were its cultivators more disposed to examine the structure of the earth, and the laws which regulate the physical distribution of its inhabitants, and less anxious to give currency to their conjectures, by endeavouring to identify them with deservedly popular truths. It would be equally favourable to the interests of Revelation, were the believer to reject such faithless auxiliaries, and, instead of exhibiting a morbid earnestness to derive support to his creed from sciences but remotely connected with his views, calmly to consider, that Geology never can, from its very nature, add the weight of a feather to the moral standard which he has embraced, or the anticipations of eternity in which he indulges, even should he fancy that it has succeeded in disclosing the dens of antediluvian hyænas, in exhibiting the skeleton of a rhinoceros drowned in the flood, or in discovering the decayed timbers of the ark. This indiscreet union of Geology and Revelation can scarcely fail to verify the censure of Bacon, by producing "*Philosophia phantastica, Religio hæretica.*"

Manse of Flisk,
19*th July* 1824.

Art. X.—*Effects of Lightning upon the Human Body, exemplified by a remarkable accident.* By Dr Tilesius, of Mülhausen.

In the ninth volume of Schweigger's Journal, there is an account of an accident, caused by lightning, which may throw some light upon the mode of action of this formidable meteor. As two carts were proceeding, in a hollow way, bordered on either side by a wood, they were successively struck by a thunder-bolt. In the first cart were seated the two brothers Teele, the one aged 33 years, the other 29: in the second, Mr Teele the

nephew, a young man of 20, and Mr Decker. The lightning struck successively the horse of the first cart, the two brothers, Mr Decker, and his companion, the latter of whom did not survive the accident. The horse was killed upon the spot; the skin of the belly was torn in all the lower part; the mouth left open; and the teeth blackened. It struck the younger Teele, passing through his umbrella, which was thrown to a distance of 24 paces from the cart; the cart itself was perforated with a hole of half a foot in diameter. The body, on being carried to the nearest village, was put into a tepid bath, and rubbed; blood flowed from the nose, the mouth, and ears, but no sign of life appeared. The mouth and nose were blackened; the skin and muscles of the arms and hands, which were both employed in holding the shaft of the umbrella, were furrowed to the bone; the sleeves of the coat and shirt were torn; but the lesions of the skin were not of the nature of tumours or scars, such as are produced by the application of red-hot iron: the skin looked as if it had been raised by a very quick rubbing. In the same manner, the clothes bore no marks of burning, but seemed as if they had been torn by the rapid passage of a sharp point. Mr Decker, who was in the same cart, received, at the same instant, so violent a blow in the lower belly, that he was precipitated from the cart, and remained senseless for half an hour. When he was undressed, the place in which he had felt the shock was of a bright red colour, but without any open wound. He was, by this time, in a condition for continuing his journey.

The two brothers Teele had suffered considerable damage from the lightning; they, however, quickly recovered, as will presently be seen. But it will be interesting, in the first place, to follow the progress of the electric fluid over the different parts of their bodies, and to observe the nature of the wounds which resulted. They were sitting by the side of one another when struck. The lightning first hit the head of the elder; it tore to pieces the velvet cap which he had on, grazed the temporal bone an inch above the left ear, and then behind the ear, after which, slightly raising the skin, it descended upon the neck, traversed the back part obliquely, and ascended toward the right ear; here it scratched the inner part of the ear, near the tragus and antihelix; it then fell upon the right shoulder, passed be-

neath the chin, over the right breast and arm; and, returning to the back, descended along the vertebral column to the sacrum. In this latter part of the course of the lightning, the skin was not cut, but only a little raised, and very red. Impressions of the same nature were seen across the arms; and attested, as well as the rupture of the clothes, the zig-zag progress of the lightning, which had passed alternately from the right side of the younger brother to the left side of the elder. It fixed upon the latter, on meeting with some pieces of metal that were in his waistcoat pocket: here it raised the skin upon a space about the size of the hand. After this, it descended upon the left part of the region of the pubis, and traversed the inner surface of the thigh, the ham and calf of the leg. A piece of steel, which the younger of the brothers carried in his fob, led the lightning to the region of the groin, where a space of the size of the piece was deprived of the skin, and affected with a deep wound. The breadth of the mark left by the lightning upon the different parts of the body was in general two inches; the wounds were more extended and deeper at the intersections of this mark; several of them were very painful, and suppurated abundantly. The skin had been rolled, in close folds, to the right and left, by the rapid passage of the lightning. The wounds did not bleed; and all that had to be done, was to provide for the renovation of the skin destroyed. In a word, there was no indication of any lesion of organs by fire or heat; but the effect produced might be compared to that which takes place when a ball grazes the surface of a limb.

Dr Tilesius having assisted at the two first dressings, had all the leisure necessary for carefully examining the form and nature of the hurts; he even took a sketch of them, which accompanies his memoir.

The brothers Teele, after having perfectly recovered themselves, were affected with violent nausea, and vomited repeatedly, when some cups of tea were given them to drink; they threw up a little blood at first, as had happened to the one who had been killed. Notwithstanding the great extent of their wounds, and their being besides of a robust habit, they had no fever. The elder was perfectly deaf on the day of the accident; but, on the following day he recovered his hearing to a certain de-

gree. No trace of paralysis made its appearance in the limbs struck by the lightning; the wounds were cicatrized in the space of a few weeks.

Dr Tilesius having seen Dr Bauer, the physician of the brothers Teele, a year after the accident (which took place in May 1821), received from him the following information. The elder has remained somewhat dull of hearing, more or less so, according to the season. He experiences a marked disposition to sleep, and would often remain 24 hours together asleep, were he not wakened. The younger has latterly had an inflammatory fever. He is subject to a periodical weakness or state of relaxation, which was before unknown to him. In general, it has had a much greater influence upon the nervous system of the two brothers than might have been presumed from the vigour of their constitution. The cicatrices of the wounds now present, in several places, the appearance of the turns of a screw.

Art. XI.—*Notice in regard to the Saline Lake of Loonar, situated in Berar, East Indies, in Lat.* 19° 10′ *N., and Long.* 75° 3′ *E.* Communicated by Cornet J. E. Alexander.

It was towards the close of a cool and delightful evening in August 1823, that I was riding leisurely along in a wooded district in Berar, and at about forty miles from the encampment of Jaulrah, in company with a small party of Mogullee horse, in the pay of His Highness the Nizam, which I had overtaken during my journeying. Whilst engaged in common-place convervation with their leader, a Duffadar, who was armed cap-a-pee, with quilted jacket, Damascus blade, spear, shield, and what not,—our discourse was interrupted, upon emerging from the shaded and gently ascending path along which our road lay, by our approach to a low and lengthened mound, the summit of which having been attained, a most romantic and interesting spectacle was presented to us.

Beneath our feet, and at the bottom of a mighty chasm, lay a deep still lake, the waters of which were slightly ruffled by the breeze, and beautifully tinted by the rays of the setting

sun. It was of a circular form, and hemmed in by an amphitheatre of cliffs, which rose, in precipitous ridges, to an elevation of about 500 feet from its shores, environing it on every side, and preventing completely the egress of its waters. The rocks which surround this interesting piece of water cannot come under the denomination of hills, for they do not, in any part, tour above the level of the surrounding country; they merely form the sides of an immense caldron, the circumference of which is about three miles. In short, the scenery, taken collectively, is a small counterpart of the celebrated Lake Avernus, differing from it in this respect, that no river

"Laco se condidit alto."

In lieu of which, a solitary spring of some magnitude dashes in a small cascade from the eastern face of the rocks, and pours its waters into an artificial stone tank, surrounded by temples and pagodas, dedicated to the god Siva; issuing from which it forms another cataract, of about 50 feet in height, before it rushes on its turbid course to join the waters of the lake.

The whole landscape, though confined, is extremely pleasing *. The dark green surface of these sunken waters strongly reflects the graceful forms of the princely Palms (*Borassus flabelliformis*, or fan-leaved), which fringe the margin, and advance into the waters of the lake their lofty stems. The sloping inclosure of rocks is covered half way up with Mango and Tamarind trees, interspersed with the *Rhododendron maximum*, or laurel-leaved Rhododendrum, which here attains a height of 10 feet. A little picturesque temple, on the opposite side of the lake from the fountain, advances its white walls to the brink. It is seldom or never visited by the inhabitants of the adjoining village, from the dread of tigers, which inhabit the jungle around it, which also forms a shelter for numerous herds of Sambers or Neel-gaes (a species of Cervus, and known in menageries by the appellation of the Horned Horse). The audacity of our small party in tasting of the waters of the lake, was looked upon by the villagers as the grossest presumption and fool-hardiness.

Superstition, always delighting in dark ideas, early and eagerly seized upon this spot, and hither she led her votaries to cele-

* See Sketch in Plate X. Fig. 4.

brate her dismal orgies. The weather-worn appearance of the buildings around the spring, sufficiently indicates that it has long been a seat of Hindoo worship. At this time, however, the small stone tank exhibited a lively and interesting sight: crowds of Mahrattah women, in a state of semi-nudity, laved their limbs in its refreshing waters; others were employed in washing their clothes, lightening their labour with singing; whilst a solitary and aged Brahmin poured his evening libation on the uncouth statue of the god.

It now remains to give some account of the waters of the lake, which, in a mineralogical point of view, are far from being uninteresting.

The name Loonar is derived from a Hindostanee word, signifying a salt-pit. The specific gravity of the water is very great. When I visited the lake immediately subsequent to the monsoon, the taste was uncommonly brackish; but in the hot season the weight of the water is nearly equal to that of Lake Asphaltites, or the Dead Sea of Judea, which is 1.246.

By a rough analysis, the component parts in 100 are nearly as follows:

Muriate of Soda, . . .	20·82
Muriate of Lime, . . .	10·60
Muriate of Magnesia, . .	6·10
	37·52

The salt of the lake is of a greyish colour, and sometimes crystallised in cubes.

Uses.—About six years ago, before the commencement of the late Mahrattah war, the annual revenue which arose from the collection of the saline crust on the margin of the lake amounted to three lacks of rupees; since which the bunds or mounds of earth, which are built across the heads of two gullies which descend into the lake, have been suffered to fall into decay: in consequence of this, a very small portion of the bed of the lake is now dry in the hot season. The town of Loonar is now almost dilapidated. When I passed through it, there was only a single doocan or shop in the Bazar, which formerly was the resort of merchants from every part of India, as the extensive karavanserahs on the outskirts of the town sufficiently indicate.

1. The chief use to which the sediment of the water was applied, was in cleaning the shawls of Cashmere, an alkaline soap being manufactured out of the muriatiferous clay, (which had the property of giving them that softness of texture which European artists are unable to imitate), and sent to that distant region.

2. It was also used as an article of food by Musselmen, and formed an ingredient in the pupree-khar or alkaline cake.

3. It was employed as aqua regia in the solution of gold; and, lastly,

4. Medicinally.

From the small portion of the bed which is now annually left dry, it is applicable to a very few of these uses *.

I have to remark, that no noxious effluvia arise from the waters, which are asserted to be unfathomable, and uninhabited by fish; but, by a strange antithesis, it is affirmed that the lake is the abode of numerous large-sized alligators.

A small tract, of a few acres in extent, consisting of rich vegetable mould, on the eastern margin of the lake, is cultivated, and employed in raising culinary vegetables.

Art. XII.—*Experiments on the Application of Professor Dœbereiner's recent Discovery, to Eudiometry.* By Edward Turner, M. D., Fellow of the Royal College of Physicians, and Lecturer on Chemistry, Edinburgh. (Concluded from p. 311.)

It appeared from some experiments related in a preceding part of this paper, (p. 101. *et seq.*) that the presence of certain gases has a positive influence in diminishing, or even preventing the action of platinum. A nearer investigation of this point was of course connected with the immediate object of the present inquiry, but was undertaken, not with this intention alone, but with the view of drawing a parallel between Platinum and Electricity relative to their action on explosive mixtures.

* The fracture of a portion of the salt which I obtained indicated an imperfectly foliated structure; it was crystallised in cubes, and had a greyish-white colour.—Edit.

Given volumes of a mixture composed of hydrogen and oxygen, in the exact proportion to form water, and quite dry, were mixed with various quantities of other gases, and submitted to the action of electricity. A strong charge from a Leyden jar did not cause detonation, when one volume of the explosive mixture was diluted with

12	vol.	of	-	-	Atmospheric Air.
14	-	-	-	-	Oxygen.
9	-	-	-	-	Hydrogen.
9	-	-	-	-	Nitrous Oxide.
4	-	-	-	-	Carbonic Oxide.
3	-	-	-	-	Carbonic Acid.
1	-	-	-	-	Olefiant Gas.
½	-	-	-	-	Coal Gas.
½	-	-	-	-	Sulphuretted Hydrogen.
1	-	-	-	-	Ammoniacal Gas.
4	-	-	-	-	Muriatic Acid Gas.
2	-	-	-	-	Sulphurous Acid Gas.

Detonation, on the contrary, was not prevented by

10	vol.	of	-	-	Atmospheric Air
12	-	-	-	-	Oxygen.
7	-	-	-	-	Hydrogen.
7	-	-	-	-	Nitrous Oxide.
3	-	-	-	-	Carbonic Oxide.
2	-	-	-	-	Carbonic Acid.
½	-	-	-	-	Olefiant Gas.
¼	-	-	-	-	Coal Gas.
¼	-	-	-	-	Sulphuretted Hydrogen.
½	-	-	-	-	Ammoniacal Gas.
3	-	-	-	-	Muriatic Acid Gas.
1	-	-	-	-	Sulphurous Acid Gas.

By reference to page 77. of Sir H. Davy's work on the safety-lamp, it will be seen, that several of my results differ slightly from his. The chief difference relates to the action of Nitrous Oxide, which he finds to have a considerably less influence than oxygen in preventing detonation; while my observation leads to an opposite conclusion. The nitrous oxide I employed was collected over mercury, was well dried, and was not used for some hours after its formation.

Sir H. Davy has satisfactorily proved, that the combustion of an explosive mixture ceases when the heat produced in the process is very rapidly removed; and he has rendered it probable, that the coexistence of carbonic acid and other gases tends to extinguish burning bodies, by the velocity with which they carry off the caloric disengaged during the combustion. It is, however, by no means clear, that this is the only efficient principle, and an investigation of the relative power of different gases in preventing the inflammation of combustible substances, would be very interesting, and might throw considerable light upon this subject. From the close analogy between the effects of Flame and of Electricity, on explosive gaseous mixtures, it must be supposed that the same causes which modify the action of the one, would likewise exert some influence on the other. Now, reasoning from the best data we at present possess, it is impossible to maintain, that the effect of different gases in preventing the action of electricity arises only from their cooling agencies. Hydrogen and nitrous oxide, whose capacities for heat, according to De Laroche and Bérard, are widely different, have an equal power of counteracting the effect of electricity. The capacities of olefiant gas and nitrous oxide bear no proportion to their relative powers of preventing explosion; and, by reference to the preceding Table, other examples of a similar nature will readily present themselves. It is therefore probable, that some other than the usually acknowledged principle is applicable in the instance of combustion, as well as in that of electricity. This view receives additional support from the following experiments.

The presence of oxygen, hydrogen, and atmospheric air, has so little power in preventing the action of platinum, that I have not been able to ascertain the exact limits at which it ceases in such mixtures.

Very different is the effect of some other elastic fluids. In mixtures composed of

	Exp. Mixt.	*Effects of a Platinum ball.*
1 vol. of Carbonic Oxide,	and 3 vol.	a Platinum ball had no effect when cold, and hardly any when warm.
1	5	Very sluggish action when cold, but good action when warm.

	Exp. Mixt.	*Effects of a Platinum ball.*
1 vol. of Carbonic Oxide,	and 7 vol.	Acted well.
1 vol. of Coal Gas. - -	3	No action when cold; a trace of action when warm.
1 - - - - -	5	Sluggish action when cold; more active when warm.
1 - - - - -	7	Acted well.
		Olefiant Gas acted nearly in the same way.
1 vol. of Sulphurous Acid Gas,	and 13	No action, either cold or warm.
1 - - - - -	18	Rapid action when cold or warm; but it ceased as rapidly as it began when a very small quantity of Gas had disappeared.
1 - - - - -	37	Acted as in the preceding case, only more gas was now consumed before the action ceased.
1 - - - - -	75	Very rapid action at first, but still all the mixture was not consumed.
		The influence of the Sulphurous Acid Gas was perceptible when it was only $\frac{1}{100}$th of the mass.
1 vol. of Sulphuretted Hydrogen,	and 19	No action either cold or warm.
1 - - - - -	29	Very sluggish and imperfect action.
1 - - - - -	59	Rapid action at first, but it ceased before all the gas was consumed.
		The influence of this gas was very perceptible when it was only $\frac{1}{100}$th of the mass.
5 vol. of Carbonic Acid Gas,	and 1	Immediate action.
8 - - - - -	1	Immediate and perfect action, but rather sluggish.
15 - - - - -	1	Still acted completely.
		Diminution of volume was occasioned when still larger quantities of Carbonic Acid were mixed with 1 vol. of the Exp. Mixture. Nitrous Oxide acted very much like Carbonic Acid. Both of these gases have certainly more influence in preventing the action of Platinum than Oxygen, Hydrogen, and Atmospheric Air, though less than the other gases.
5 vol. of Muriatic Acid Gas,	and 1	Very slight trace of action.

	Exp. Mixt.	*Effects of a Platinum ball.*
3 vol. Muriatic Acid Gas,	and 1 vol.	Diminution of vol. to full extent, but sluggish action.
1 - - - - -	4	Rapid and perfect action; but in the intermediate proportions, the ball acted sluggishly, although perfectly.
1 vol. of Ammoniacal Gas,	and 3	No action cold, but with considerable energy when warm.
1 - - - - -	5	Acted sluggishly, but diminution was to the full extent.
1 - - - - -	9	Rapid and perfect action.

The explosive mixture was, as usual, formed of 2 volumes of hydrogen to 1 of oxygen. By the term Warm is meant, that the ball was put into the gas when just so hot as to be borne on the palm of the hand. The most active ball which I could make was employed in these experiments; when a weaker ball was used, but one which nevertheless acted with energy on the pure explosive mixture, the counteracting effect of several gases, as carbonic oxide, olefiant and ammoniacal gas, was far greater than is stated in the preceding Table.

This singular property of certain gases can by no means be attributed to their cooling agency, for platinum acts on an explosive mixture of oxygen and hydrogen, even when quite cold; nor will the mere circumstance of dilution account for the phenomena. Heat is always generated by the chemical changes produced, and this heat, in ordinary circumstances, greatly augments the energy of the platinum; but mere abstraction of caloric could obviously have no other effect than to cause a slower action. We must therefore admit the operation of some other cause, which is at present quite undetermined; and, as the same gases which prevent the action of platinum, likewise control the influence of electricity, it seems rational to infer, that the same power operates to a certain extent in both cases.

I have hitherto made no allusion to the principle by which platinum acts in causing the combination of oxygen and hydrogen gases. The investigation is so obscure and difficult, that it can only be attempted with a prospect of success after the accumulation of many facts; nor do the experiments I have hitherto made justify the decided adoption of any opinion. Prof.

Dœbereiner imagines that galvanism is the principle concerned; that, by putting platinum into an explosive mixture, we establish a simple galvanic circle, in which the hydrogen represents the zinc. That ingenious chemist has not, to my knowledge, advanced one argument in proof of his assumption, and has been led to it purely, as I imagine, by the impossibility of explaining phenomena on any other known principle. It is, however, better in science to account for phenomena on known principles, than to search unnecessarily for new; and, as galvanism does afford us a rational explanation, while we have no positive proof that it is not the agent concerned, we can hardly refuse a certain degree of belief to the ingenious supposition of Prof. Dœbereiner. The facts which have been stated in the present paper give additional plausibility to this view; for, on taking a comparative survey of the two Tables, which show the effect of gases in preventing the action of electricity and platinum, a singular coincidence will be observed, which certainly cannot be the result of chance. One or two exceptions do indeed occur, but these are far outweighed by the instances which are favourable to this side of the question, and which may be adduced in support of it. It must, on the contrary, be confessed, that all my attempts to render electrical excitement evident, by means of an electrometer, have wholly failed. This negative argument does not, however, merit much confidence, because the copious production of water forms such a conducting atmosphere around the metal, as effectually to prevent the most delicate electrometer from being affected.

Before concluding this paper, I shall mention a few detached facts, which may not be uninteresting to the Society, and which will show what precautions are necessary for ensuring the regular action of platinum.

On exposing pure spongy platinum to the air and dust of an ordinary sitting-room, the metal gradually loses its peculiar property. After three days it still became luminous from a jet of hydrogen, but at the end of six days no action ensued, not even when it was put into a mixture of oxygen and hydrogen. By simply igniting the platinum, its energy was completely restored. A piece of platinum was kept in a closed drawer of the same

apartment, in such a manner that it was freely exposed to the air, but protected from dust. At the close of a fortnight, it still set fire to hydrogen, though its energy was somewhat diminished. Some of the metal was kept a month in a dry bottle, furnished with a glass-stopper, and then acted quite well. A platinum ball lost and retained its energy under similar circumstances.

A piece of active platinum invariably failed to give light, if kept 24 hours on mercury, the effect being the same whether it was covered by an inverted jar or not. On keeping an active ball in contact with mercury during two hours, its energy was perceptibly impaired. The action of mercury is still more injurious, when a heated ball is plunged under it, and held there during a few seconds.

Spongy platinum absorbs water greedily when any part of it is dipped into that liquid, and then does not become luminous under the jet of hydrogen. A platinum ball absorbs water in like manner, and in consequence loses its activity. On putting a ball thus moistened into mixed oxygen and hydrogen gases, an exceedingly sluggish action ensued, during which a large quantity of water condensed on the glass. By removing the ball successively into fresh portions of the explosive mixture, it gradually lost much of its moisture, and regained a great part of its former energy, which was not, however, completely restored till it had been ignited. Similar to this was the effect of moistening a ball with alcohol and ether, only the activity returned more speedily under the same treatment. Spongy platinum, if moistened with very strong sulphuric ether, did become luminous under a continued jet of hydrogen, but when alcohol or water was used it did not. A platinum ball moistened with sulphuric, nitric, and muriatic acids, did not act at all on an explosive mixture, but ignition restored its activity.

When platinum is kept for hours in dry oxygen, hydrogen, or atmospheric air, it acts afterwards on an explosive mixture with its usual activity. An active platinum ball was kept in carbonic acid during an hour, and afterwards acted readily on an explosive mixture. The same ball was put successively into olefiant gas, carbonic oxide, and coal gas, for five minutes, and suffered a considerable abatement of its energy in consequence, but still acted. Put into muriatic acid gas, for the same space of

time, it acted still more sluggishly. Kept for the same time in sulphurous acid, sulphuretted hydrogen and ammoniacal gas, it had completely lost its energy, for it occasioned no diminution of volume when brought in contact with the explosive mixture.

Spongy platinum was pressed between two pieces of clean metal, till it had become a compact leaf of platinum. Its effect on a jet of hydrogen was tried at different periods of the process. So long as any of the porous texture remained, it still became luminous; but when this was wholly destroyed, it gave no light whatever. A very gentle heat still enabled it to become luminous, and to inflame the hydrogen. I have repeated with success the experiments mentioned by MM. Thenard and Dulong, of throwing a jet of hydrogen on a fold of platinum foil, and on a coil of the wire. The metal under this form requires to be sharply heated for this purpose, to a degree apparently beyond that at which mercury boils.

The lamp without flame, proposed by Sir H. Davy to be formed with a coil of platinum wire, may be conveniently made with spongy platinum. The porous mass need only be supported just above, but very close to the wick, by means of a small wire; and when the spirit-lamp is allowed to burn for a few seconds, and is then blown out, the metal becomes red, and continues so till the alcohol is consumed. By this means a considerable body of light may be obtained, by which one can read small print, or see even the seconds hand of a watch with perfect precision. Sir H. Davy proposes to suspend a coil of platinum within the safety-lamp, in this expectation, that, should the atmosphere of a mine become at any time so charged with fire-damp as to extinguish the lamp itself, the platinum, by maintaining a slow combustion, would afford a certain degree of light to the miner. Should this be really useful, still greater advantage would probably be derived from suspending spongy platinum, inclosed in a little wire cage of the same metal within the safety-lamp, as the mass of light would then be increased.

The product of the slow combustion of alcohol by means of the porous platinum, is only carbonic acid and water; I have not at least detected either the acetic or any other acid.

Art. XIII.—*Account of William Dempster, who swallowed a Table-knife nine inches long; with a Notice of a similar case in a Prussian Knife-eater.* By Thomas Barnes, M. D. Member of the Royal College of Surgeons, London, Member of the Wernerian Natural History Society, and Physician to the Fever Hospital and Dispensary, Carlisle.

Several cases of knife-eaters are on record. One of the most remarkable is that of John Cummings, who lived ten years after having swallowed a number of clasp-knives. His case is related by Dr Marcet, and an account of it is published in the seventh volume of the Edinburgh Philosophical Journal. The following case lately occurred in Carlisle, and excited considerable interest and sympathy, not only among the inhabitants of that city, but also very generally throughout the country. The case was particularly interesting to the medical profession, both as to the physiological fact, that the functions of life were not interrupted, and even suffered little disturbance by the presence of so large an extraneous substance as a table-knife in the stomach; and as to the medical and chirurgical treatment the profession could supply in so singular a case.

William Dempster, a juggler, twenty-eight years of age, of a high complexion and sanguine temperament, came to Carlisle in November last, with the intention of exhibiting some tricks by slight of hand; and on the evening of the 17th of the same month, when in a small inn in Botchergate, with a number of people about him, whom he was amusing, by pretending to swallow a table-knife; and in the act of putting the knife into his throat, he thought some person near him was about to touch his elbow, which agitated and confused him so much, that the knife slipped from his fingers, and passed down the gullet into the stomach. Immediately after the accident, he became dreadfully alarmed,—was in great mental agony, and apprehended instantaneous death. The knife, when given to him, measured nine inches in length, and had a bone handle, which went first down into the stomach: the blade, which was not very sharp, was one inch in breadth. Medical assistanee was soon procured, and several attempts were made to extract the knife

first, with the fingers alone, then with a pair of short curved forceps, and afterwards by a pair of very long forceps, made for the occasion, but without success. The knife, indeed, could not be reached by any of these means; and nothing resembling it could be felt externally on the region of the stomach. His mind continued much depressed, though he had very little pain or uneasiness. He was encouraged by the medical attendants, and directed to be removed as quietly as possible to his lodgings, and to take nothing that night except a little cold water. He had some sleep, and next morning said, he felt occasionally pain in his stomach; ℥xii. of blood were taken from his arm, and an enema was ordered him. He afterwards complained of pain in the left shoulder, shooting across the chest to the stomach; and the blood-letting was repeated. A hard substance, which was believed to be the handle of the knife, could now be felt very distinctly, by pressing the fingers very gently on the umbilicus; slight pressure gave him considerable pain. Although his suffering was much less than could have been expected, his health became gradually impaired, and his strength reduced. He was able to walk about a little in the day, and could sleep in the night on his back, but could not lie on either side. He took some diluted sulphuric acid for two or three weeks, which was discontinued, as he thought it increased the pain in his stomach. His bowels were kept open by castor-oil and injections; the alvine evacuations were of a dark ferruginous colour, which probably arose from the decomposition of the knife; the pulse was very little affected, being generally between 70 and 80 in a minute. His diet consisted of soup, gruel, and tea, taken in small quantities. When the stomach was empty of food, the handle of the knife could be distinctly felt, extending from above downwards, by placing the hand very lightly on the abdomen, a little above the umbilicus; but a single cup of tea, or a little food of any kind, distended the stomach so much, that it entirely disappeared. He was frequently squeamish and sick at his stomach, and sometimes felt a severe twisting pain in that organ.

The case being a remarkable one, and of very rare occurrence, the patient was visited by a great number of medical men. All the professional men in Carlisle were consulted respecting him; and that nothing might be omitted that could benefit this unfortu-

nate man, his case was stated to Sir Astley Cooper of London, Mr George Bell of Edinburgh, and a few others. As the great length of the knife would prevent the possibility of its passing the pyloris, or making the turns of the intestines, and it seemed improbable that the patient would live sufficiently long for it to be dissolved in the stomach, various means were suggested to extract it; for, although Dempster had survived the first shock of swallowing the knife, and there was no risk of speedy destruction of life, the action of the gastric juice, or of any medicine that could be given, it was supposed, would be so slow, particularly upon the blade of the knife, that it was deemed advisable to extract it, if possible. Besides the means already mentioned, the following, though not had recourse to, are deserving of notice. An eminent and excellent surgeon recommended, that Dempster should be accustomed to receive two or three times a-day, a large smooth elastic gum bougie into the stomach, and gradually allow it to remain one, two, three, or ten minutes there; that tubes of elastic gum, twenty inches long, should be prepared, of several sizes, from one-quarter of an inch in diameter, and open at each end; that the extremity to be introduced into the stomach, should be filled up by means of an ivory ball attached to a wire or piece of whalebone, so that the lining membrane of the œsophagus would not be injured during its passage. The piece of ivory to be removed, and instead of it a pair of forceps, resembling those used by Sir Astley Cooper for removing stones from the bladder, or a pair of forceps which expand of themselves when pushed forward, and not restrained by the tube, to be introduced. The same surgeon observed, that many contrivances might be devised for laying hold of, or entangling the knife; but as it might be seized at an improper place, no instrument should be used but which could be immediately disengaged from the knife if necessary. The above plan was recommended on the principle, that, when a probang, or any foreign instrument, is forcibly introduced into the stomach, through the œsophagus, violent exertions and spasms of the muscles concerned in deglutition take place; but on every successive repetition, these spasms become less and less, until they abate almost entirely. This is exemplified in those who require to be fed by a tube, introduced into the

stomach, and a syringe. Another plan, was to make the knife force its own way through the parietes of the stomach and abdomen, and to assist it ultimately by a chirurgical operation. This was to be accomplished by the patient's lying entirely on one side, or on his face, when his stomach was empty, so that inflammation and suppuration might be excited; and after adhesion had taken place, might be aided by the scalpel. We know, that a mulberry calculus has repeatedly forced its way through the bladder and rectum, through the bladder and perinæum, through the bladder above the pubes, by the patient having been confined for years to bed, and lying in the relative postures favourable for such operations of nature; also, that many gall-stones have forced their way through the abdominal parietes. The only other plan of treatment that I shall mention, is that which was proposed by the surgeons of the Carlisle Dispensary, and was also recommended and sanctioned by one of the first surgeons in Europe; it was, that an incision should be made into the patient's stomach, and the knife extracted. The last report of the Carlisle Dispensary contains the following observations concerning Dempster. "The surgeons of the Dispensary were unanimously agreed as to the best mode of treating this extraordinary case: they were of opinion, that nothing but an operation could save the patient's life, but he could not be persuaded to submit to it." He remained in Carlisle until the 28th of December, when he left it, with the intention of proceeding to his friends at Hammersmith, in the neighbourhood of London. It is proper to remark, that his journey was neither recommended nor sanctioned by the medical officers of the Dispensary; it was contrary to their advice; they apprehended dangerous and fatal consequences from it, and anxiously wished him to continue in Carlisle. It appears from the public prints, that what they apprehended has, in reality, happened. This unfortunate man was prevented from pursuing his journey further than Middlewick in Cheshire, where he died on the 16th of January. Inflammation and gangrene of the stomach having been produced by the irritation of the knife and the jolting of the conveyance in his journey." The celebrated surgeon above alluded to, who recommended an operation, stated, that he was decidedly of opinion, that an incision should be made into the person's

stomach, if the handle of the knife could be felt as the director to the surgeon. The incision should be in the direction of the linea semilunaris. The stomach should be previously quite empty of food and of liquids. The patient was to be supported for ten days after the operation, by glysters of broth and jelly; after ten days, jelly might be given.

As Dempster died at a considerable distance from Carlisle, and no authentic account of the dissection has been published, I am not aware of what change the knife had undergone by being retained in the stomach, nor of the precise appearances the abdominal viscera exhibited after death. The present communication, it is hoped, will induce the surgeons who examined the body, to give an account of their dissection to the public.

A case very similar to the above occurred in Prussia in 1635, of which a very interesting account was written in Latin, by Dr Daniel Beckher of Dantzic, and published at Leyden in 1636. The case is well authenticated. Beckher's account of it was submitted to the Faculty of Leyden, who affixed their names to their criticism of his work. It received from them unqualified praise. They passed many high encomiums on the author; considered the case singular, the cure miraculous, and the history of it faithfully and accurately detailed. The style of the work is elegant aud classical; the case is described with much minuteness, simplicity and clearness, and is accompanied with many excellent and valuable observations. The book is divided into four sections. The first treats of the swallowing of the knife; the second, the consultation of the Faculty; the third, the incision of the abdomen and stomach, and the extraction of the knife; and the fourth, the healing of the wound. The following is an abstract of the case. On the morning of the 29th of May 1635, Andrew Grunbeide, a young peasant, feeling sick at stomach from having committed some irregularity in his mode of living, endeavoured to excite vomiting by irritating the fauces with the handle of a knife, but the desired effect not being immediately produced, he thrust it further down, in consequence of which it escaped his hold, and gradually descended into the stomach. The knife-eater was terribly frightened at the time, and continued afterwards much depressed, yet was able to follow his accustomed employment without much inconvenience.

The wretched condition of this afflicted peasant excited much pity, and many physicians and surgeons of great learning and celebrity were consulted respecting him. At a meeting of the Faculty, held on the 25th of June, it was decided, that the abdomen should be opened, an incision made into the stomach, and the knife extracted. Previous to the operation, the patient was to make use of a balsamic oil, called Spanish Balsam, which they supposed would alleviate the pains of the stomach, and facilitate the healing of the wound. The 9th of July was fixed for the operation; and it was performed in the presence of the Dean of Faculty of Medicine, the Physicians and Members of College, the Students of Medicine, and an experienced surgeon and lithotomist of the name of Shoval. A straight incision was made in the left hypochondrium, two finger breadths under the false ribs; first through the skin and cellular membrane, then through the muscles and peritonæum. The stomach subsided and slipped from the fingers, which prevented it from being immediately seized; but it was at length caught hold of with a curved needle, and drawn out of the wound. A small incision was then made into it upon the knife, which was then easily extracted. The stomach immediately collapsed. After the external wound had been properly cleaned, it was united with five sutures, and tepid balsam poured into the interstices. Tents impregnated with the same balsam, and a cataplasm composed of bolar earth, the white of egg and alum, were then applied. In the evening, the cataplasm was removed, a styptic plaster applied, and he took a decoction of betony, tormentil, and feverfew, with a powder, consisting of nutmeg and crab's-eyes. The report next morning was, that he had had a quiet night; pulse a little accelerated; had passed some bloody urine, which deposited a sediment of grumous blood; the wound looked well; he complained of no pain; two sutures were removed, and the balsam and plaster again applied. He was allowed chicken-broth, boiled with some bitter and astringent herbs. The wound was dressed again in the evening; and as he had not had an alvine evacuation, a suppository was directed to be used. On the 11th, two more sutures were removed; pulse less frequent; urine still bloody; complained of pain and tension in the left hypochondrium. Two enemas were administered, and he passed a

copious black dejection. The wound was regularly dressed twice a-day; he was allowed a very strict diet, and his drink was moderately warm: bowels were kept open by injections; urine continued tinged with blood until the 13th. On the 15th of July, and the 7th after the operation, he was pronounced out of danger. On the 16th, he took a little infusion of rhubarb, with syrup, as an aperient. The same treatment and dressing were continued until the 23d of July, which was the 14th day after the operation, when the wound had healed, and nothing occurred afterwards worthy of notice. He was restored to the best of health,—gradually returned to his ordinary diet and employment, and never afterwards complained of pains in his stomach. In the fifth volume of Jones's edition of the Philosophical Transactions, Dr W. Oliver informs us, that, in 1685, he was at Koningsberg in Prussia, where he saw the knife that was swallowed by the Prussian boor. It was kept in a velvet-bag in the King of Prussia's library. According to the engravings given of it he says, it measured six inches and a-half long, English measure. Beckher says, the knife "decem pollicum latitudinem adæquabat." Dr Oliver met with a Mr Taylor, a Scotch merchant at Koningsberg, who told him, that Andrew Grunbeide was his particular friend and acquaintance; that he saw his wound several times when his surgeons dressed him, and was godfather to one or two of his children afterwards.

It is much to be regretted, that William Dempster could neither be prevailed upon to submit to an operation, nor to remain in Carlisle. As an operation succeeded near two centuries ago, when surgery was in a very imperfect state, it is highly probable, that, under the present improved state of surgery, a similar operation would have been attended with success. The many valuable improvements that have been introduced into surgery, both in the operative part, and in the subsequent mode of treatment, must give the moderns a decided advantage over the ancients, in the success of their operations. Had he remained in Carlisle, even although no operation had been performed, it is very probable, his life would have been spared much longer than it actually was. He became weak and emaciated; but, as has been before stated, was able to walk about the town, and the stomach had, in some degree, become accustomed to

the presence of the knife. The handle, and perhaps the blade also, would be dissolving, so that the bulk would be diminished; and if the knife had not been altogether removed in this way, it would have produced less irritation, and he might have lived a considerable time. There is even some probability that the knife might, in the course of time, have made its way through the stomach and parietes of the abdomen, by inflammation, abscess and ulceration, as extraneous bodies have been frequently brought from various internal parts to the external surface, by these processes, or by what some surgeons have termed *progressive absorption.*

CARLISLE, *March* 1824.

ART. XIV.—*Account of some remarkable Hail-Stones.*

IN the second part of the eleventh volume of the *Nova Acta Physico-medica Academiæ Cæsareæ Leopoldinæ Carolinæ Naturæ Curiosorum*, Dr Nögerath informs us, that, on the 7th May 1822, a tremendous hail-shower fell in and around Bonn. Its violence was so great, that not only the glass of all the windows in the district where it fell was broken, but even the roofs of the houses were shattered, and the hail-stones were so forcibly propelled, that the slates on the roofs were perforated as if by musket-bullets. Many men were wounded, numbers of wild and domesticated animals were killed, and the vineyards suffered much. The general size of the hail-stones was about one inch and a half in diameter, with a weight of nearly 300 grains. When whole, which was not generally the case, the general outline was elliptical, or compressed globular, and the form *cerebral*, or resembling the brain of a warm-blooded animal, in which the membranes had been removed. More frequently the form was lenticular, and appeared polished on the two ends, as if by friction. The masses had a concentric lamellar structure in the centre was a white, nearly opaque, nucleus, of a round or elliptical form, around which were arranged concentric layers, which increased in translucency from the innermost to the outermost. They at the same time exhibited a beautiful stellular fibrous arrangement, caused by rows of air-bubbles disposed in

PLATE IX.

DIFFERENT KINDS OF HAIL-STONES

Fig. 3

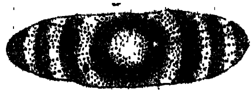


Fig. 1.

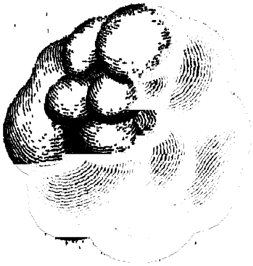


Fig. 2.

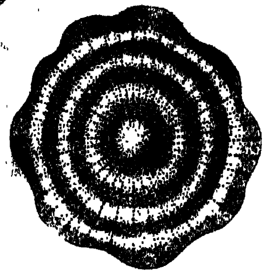


Fig. 4.

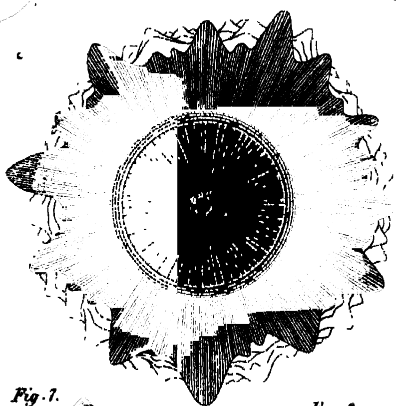


Fig. 5

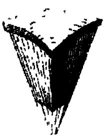


Fig. 7.

Fig. 8

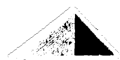
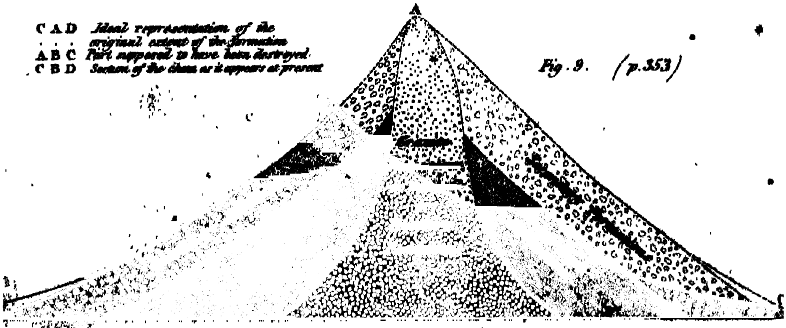


Fig. 6.

C A D Ideal representation of the original extent of the formation
A B C Part supposed to have been destroyed.
C B D Section of the chain as it appears at present

Fig. 9. (p. 353)



Transverse Section of the chain of the Pyrenees intended to show the degradation it is supposed to have experienced.

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Published by A. Constable & Co. Edinr [illegible]

radii. Fig. 1. Pl. IX. represents the cerebral form of the hail-stones. Fig. 2. is the section of a hail-stone, shewing the concentric lamellar structure and the beautiful radiation. Fig. 3. is one of the hail-stones cut in the direction of a shorter axis.

Captain Delcross, in the 13th volume of the *Bibliotheque Universelle*, describes hail-stones having the concentric lamellar structure and stellular fibrous arrangement. Fig. 4. is a section of one of the hail-stones observed by Delcross. In it the concentric lamellar structure is not so distinctly shewn as in the figures given by Nögerath, owing to the circumstance of the drawing having been made by candle-light, when the structure is not so well seen. In the hail-stones examined by Delcross, the surface, as represented in Fig. 4., was provided with pyramidal forms, whose summits are already blunted by incipient melting; whereas, in the hail-stones examined by Nögerath, the surface was covered with irregularly grouped segments of balls. When the edges and angles of the pyramids are melted down, the cerebral form is produced; when the masses of hail, having the structures described, burst asunder, the fragments have a pyramidal form, as represented by Figs. 5. and 6., and then forms what has been described, under the name *Pyramidal Hail.*

In a letter addressed to P. Neill, Esq., by Robert Lindsay, Esq. of Aberdeen, and read before the Wernerian Society on 29th November 1823, there is an interesting account of a hail-storm, in which some of the hail-stones had a pyramidal form. The following is an extract from the letter:

"During Thursday the 27th of June (1823), the sky had, for the greater part of the day, been overcast with large compact white clouds, such as in winter are thought to threaten snow, in summer thunder, and which have been very frequent during what is past of this cold summer with us. There was also a lower range of a darker hue, which had given us a few slight showers, and some large hail-stones. The wind had blown for a number of days from NW. or thereabout. Between 1 and 2 P. M. it veered about by W. to SE. The clouds, however, continued throughout the evening from NW.

"At 5 P. M. fell a shower,—at first large drops of rain as formerly, and in a short time a violent hail-shower. The regula-

rity in the shape of the hail-stones, and their hard and peculiar consistence, seemed to entitle them to some notice. They were included, almost universally, each by five sides or surfaces, four plane, constituting the sides of an irregular pyramid, and one spherical in place of a base. (Pl. IX. Fig. 7, 8.) The length of the longest line did not exceed $\frac{1}{2}$ inch in any instance that I observed; but as the hail-stones dissolved very fast, and as they were indeed, for the most part, rounded in the angles before I found them, it is probable they had been larger at first. I however examined them the moment they fell. The spherical surface appeared, to the depth of $\frac{1}{20}$th or $\frac{1}{30}$th of an inch, to be solid, as it was transparent. The rest of the hail-stone was opaque, consisting of crystals or minute columnar forms, perpendicular to the spherical surface. The transparency of this last could not have been occasioned by the absorption of water, as, although the hail-stone was equally moist all over, no part was transparent but this.

"Not having access to a barometer or thermometer at the time, I cannot say what indications such instruments would have afforded.

"The wind was gentle. The lower clouds, what are called the *Scud*, were frequently deranged (this was at $5\frac{1}{2}$ P. M.) and altered in their forms by collision with the lower current of air from the SE., which (probably by its greater heat) in many instances, dissipated the parts of the lower clouds, so as that the parts seemed to repel each other. Columns also of warm air were apparently making their way at this time up through the low-breaking clouds in the most distinct manner.

"If I might be allowed to hazard a conjecture, which I think neither extravagant nor unfounded, I should be of opinion, that the hail-stones had, in their formation, assumed the same oblate spheroidal shape I observed in those in Orkney *, in 1818, and that by their splitting in the lower regions of the atmosphere, the form was produced that I have decribed. The regularity in the form † may have been occasioned

* *Phil. Trans. Edin.* vol. ix.

† Some hail-stones appeared to have only three plane sides distinct.

by laws of fracture in *crystallised* bodies, which interfering circumstances at the surface of the earth prevent us from observing *clearly*. The probable cause of fracture may be found in the expansion of air in the opaque nucleus, in electricity, or in centrifugal force. The first of these I should think perfectly equivalent to the force required, if it be assumed that a small degree of heat may be transmitted by the solid surface from without."

ART. XV.—*Analysis of the Radiated Celestine, from Norten, near Hanover.* By EDWARD TURNER, M. D. Member of the Mineralogical Society of Jena, Fellow of the Royal College of Physicians, and Lecturer on Chemistry, Edinburgh.

THE subject of this analysis was examined a few years ago by M. Gruner of Hanover *, who found it to differ from all other known species of celestine, in containing a large quantity of the sulphate of baryta. M. Gruner, who has given an account of its geognostical relations, describes it as occurring in a coarse-grained flœtz limestone, in which it forms three distinct parallel beds. In the first bed, the mineral is fresh; in the second, it has a weathered appearance; and, in the third, though the radiated texture is still visible, it is in a state of such complete disintegration, as to fall into an earthy, chalky powder, from the least pressure. M. Gruner's analysis gives for a result, in round numbers,

Sulphate of Strontian, ...	73
Sulphate of Baryta, ...	26

The weathered variety, on the contrary, was found to contain,

Sulphate of Baryta, ...	74
Sulphate of Strontian, ...	24

The unusual peculiarities of this celestine induced me to subject it to analysis, not as doubting the accuracy of M. Gruner's observation, but with the desire of investigating the process of

* See Gilbert's Annalen, vol. lx.; or Annals of Philosophy for 1820.

disintegration; and Professor Stromeyer, in whose laboratory I operated, kindly furnished me with some very pure specimens of the mineral for that purpose.

The fresh celestine is translucent, of a white colour, with a shade of blue in some parts, and a vitreous lustre. Its texture is distinctly radiated, and crystalline; it has not the least appearance of being a mixed mineral, but is homogeneous throughout. Its hardness is inconsiderable, being very nearly that of the common celestine, and it is easily reduced to powder. Its specific gravity, at 73° Fahr. and 30 inches bar., I found to be 3.7619.

A. 100 parts of the fresh celestine, in small fragments, were kept in a red heat for half an hour. They decrepitated strongly, and formed a coarse powder, which was tinged red by numerous minute points of the peroxide of iron. The whole loss amounted to 0.3.

B. Small pieces were put into a dry glass-tube closed at one end, and heated over the flame of a spirit-lamp. They again decrepitated even before the glass was heated to full redness, and the powder became red at the very instant of decrepitation; at the same time, a small quantity of water condensed on the cold parts of the glass, which had acid properties, for moistened litmus-paper, put into the tube, received a permanent red stain. The acid was carefully removed by pure water, and proved to be the sulphuric, by the test of baryta. A faint rather empyreumatic odour was likewise perceived, the exact character of which could not from its faintness be determined. It probably indicates the presence of a minute portion of bituminous matter, which is also present in some other species of celestine.

C. 100 parts of finely powdered celestine, well dried at a temperature of 212° Fahrenheit, were calcined during 20 minutes. They had lost only 0.05, and were coloured from iron.

D. 100 parts of the fresh uncalcined celestine, in powder, were digested in pure water during four days, when they had lost 0.6, and the water contained sulphuric acid, strontian, and iron. Fragments of fresh celestine were put into water, and in the course of 24 hours they had lost their whiteness, becoming

stained of an ochre colour. The water contained sulphuric acid and iron, even before any sulphate of strontian was dissolved, and an ochre-coloured sediment gradually subsided, which was a subsulphate of the peroxide of iron.

On digesting the calcined celestine in water, no trace of iron was discovered.

The absence of the sulphuret of iron was proved by the action of strong muriatic acid on the calcined and uncalcined mineral; for no trace of sulphuretted hydrogen could be detected by the acetate of lead, even when the mixture was warmed.

It follows that the fresh celestine contains a minute portion of the protosulphate of iron, the presence of which, though in very small quantity, gives a strong tendency to disintegration. From the combined action of air and moisture, this salt is first dissolved, and afterwards converted into the subsulphate of the peroxide, causing the ochre stains, which appear as soon as decomposition has commenced. The water now acts by dissolving the sulphate of strontian, small portions of which are successively removed. What takes place in the laboratory, will of course occur in the earth, when favourable circumstances present themselves; the free access of water must in like manner occasion decomposition, with gradual removal of the sulphate of strontian, till the structure of the mineral is destroyed, and the original proportion of its constituents completely inverted.

E. *a.* 4.085 grammes of finely powdered fresh celestine were calcined for half an hour, with 10 grammes of the subcarbonate of soda, and were found, on cooling, to have fused into a grey enamel. The ignited mass was digested in pure water, filtered, and thoroughly edulcorated. Muriatic acid was added, in slight excess, to the clear alkaline solution, and the sulphuric acid precipitated by muriate of baryta. The sulphate of baryta, collected, dried and ignited, weighed 4.9775, indicating 1.6924 of real sulphuric acid.

b. The solution from which the sulphate of baryta had been separated, was freed from baryta by Glauber-salt, and was brought carefully to dryness. No silica nor alumina was found, but a little oxide of manganese was procured by the addition of an alkali.

c. The earthy carbonates dissolved completely in muriatic acid, and the solution was evaporated to perfect dryness. Water took up every thing but a little submuriate of iron, and the neutral solution was set to crystallise by spontaneous evaporation. The tabular crystals of muriate of baryta were now seen in considerable number, though less in quantity than the prisms of the strontian salt. The mixed crystals were digested in strong alcohol. The muriate of baryta, which remained on the filtre, was redissolved in water, and made to crystallise again; by which measure I was satisfied that the muriate of strontian had been wholly removed. The baryta was precipitated by solution of Glauber-salt, and the sulphate of baryta, after calcination, weighed 0.8335, indicating 0.5502 of pure baryta.

d. The alcoholic solution was diluted with water, and the alcohol removed by distillation. The absence of the muriate of baryta was shown, by allowing the solution to crystallise. To the heated solution of muriate of strontian pure ammonia was added, when oxide of manganese, with a little iron, separated. After rapid filtration the strontian was precipitated, while still hot, by carbonate of ammonia. The carbonate of strontian collected, and sharply dried, weighed 2.602, of which 0.012 was carbonate of lime. Pure carbonate of strontian amounted therefore to 2.59, indicating 1.817 of the pure earth.

e. The carbonate of strontian was dissolved in pure nitric acid, and the solution brought to perfect dryness. It was now digested in the strongest alcohol, which dissolved a minute portion of the nitrate of lime. The lime was separated by oxalate of potassa, and 0.007 of pure lime obtained, equal to 0.012 carbonate of lime, to be subtracted from the carbonate of strontian.

A second analysis gave a result very nearly corresponding with the preceding. On this occasion, the iron and manganese were separated, in the first instance, from the combined muriates of baryta and strontian, by pure ammonia. The iron was separated from the manganese by succinate of ammonia, and amounted, when calculated as the protoxide, to 0.168 per cent. The manganese, as well that obtained from the earthy muriates, as that held in solution by the soda, amounted to 0.15 per cent. of the deutoxide.

The fresh celestine is therefore composed of

				per cent.
Sulphuric Acid,	1.6924	.	41.431
Baryta,	0.5502	.	13.469
Strontian,	1.817	.	44.481
Lime,	0.007	.	0.171
Protoxide of Iron,		.	0.168
Deutoxide of Manganese,		.	0.15
	4.0566	.	99.870

Considering the sulphates of strontian and barytes to be the essential compound, the mineral is composed of

Sulphate of Strontian,	78.205
Sulphate of Baryta,	20.41

Supposing it to be a compound of one atom of the sulphate of baryta, with five atoms of the sulphate of strontian, it would be composed of

Sulphate of Strontian,	79.99
Sulphate of Baryta,	20.41

The crystalline nature of this mineral, and its homogeneous aspect, lead to the presumption that it is a definite compound of the sulphates of baryta and strontian. The weathered variety, on the contrary, can only be regarded as a disintegrated mineral, and must be variable in the proportion of its constituents, according as the process of disintegration is more or less advanced. I have seen specimens in all stages of decomposition, from its commencement, when the mineral was firm, and only stained with the subsalt of iron, till it was so friable as to fall into a powder from the least touch. A weathered portion has frequently a firm nucleus within, which has undergone hardly any change.

The specific gravity of the weathered celestine is much less than that of the fresh variety. The former has lost all its lustre, and is quite opaque. Some parts are very white, while others are strongly stained with iron. All the specimens I examined uniformly effloresced with muriatic acid, which the fresh mineral did not; carbonic acid was disengaged, and a muriate of lime was formed. The carbonate of lime must have been deposited by water, for it pervaded the mass irregularly, some portions, even from the same specimen, containing more than others. A very pure looking and completely disintegrated specimen, given

me by Professor Hausmann of Göttingen, was reduced to powder, and digested a few minutes in dilute muriatic acid, to remove the carbonate of lime. The dried powder was analysed in the manner already detailed, and, neglecting traces of alumina, iron, manganese, and lime, 100 parts yielded,

Sulphuric Acid, ...	37.172
Baryta, ...	40.995
Strontian, ...	20.72
	98.887

Or, calculating from the capacity of the two earths,

Sulphate of Baryta, ...	62.114
Sulphate of Strontian, ...	36.431
	98.545

In the preceding analyses, I adopted the advice of Professor Stromeyer, of digesting the crystallised, and not the dried, muriates of baryta and strontian in alcohol. Muriate of strontian, after having lost its water of crystallisation, does not dissolve readily in stiong alcohol; and the heat, which it is then necessary to employ for effecting its perfect solution, is apt to cause some muriate of baryta to be likewise dissolved. The crystallised muriate of strontian, on the contrary, dissolves readily in alcohol, at a temperature of 70° or 80° Fahrenheit. The addition of a few drops of concentrated muriatic acid I found to be very useful in this process; for it did not interfere with the solubility of the muriate of strontian, while it prevented the solution of the barytic salt.

Art. XVI.—*Some Account of an Animal of the Genus* Bos, *which in India is named* Gour *. By Thomas Stewart Traill, M. D. F. R. S. E., M. W. S., &c.

Notwithstanding the extensive intercourse which has for ages subsisted between the western world and India, it is rather

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* The Gour of India is a very different animal from the Gour of Persia. The latter is the Wild Ass; an animal remarkable for its fleetness. It was formerly a grand object of the chace, with the Sassanian monarchs of Persia.

surprising, that the natural history of some of the larger animals of those celebrated regions should yet be imperfectly known to naturalists in Europe. This obscurity seems to have especially involved the animals of the bovine genus. Naturalists are now generally agreed in considering the Indian cattle with the remarkable protuberance on the shoulder, as mere varieties of our domestic ox; but an exaggerated account of the Wild Buffalo of India, not many years ago, induced Kerr, in his unfinished translation of the *Systema Naturæ*, to introduce it as a distinct species, under the name of *Bos Arnee*. In this he was followed by Shaw; but the error was exposed by Mr Colebrooke in 1805; and Cuvier, with much propriety, has considered that animal as merely a variety of buffalo. My inquiries on this subject lead to the same conclusion. My friend Captain Rogers, of the Bengal Army, who has paid considerable attention to the quadrupeds of India, states, that the domestic Buffalo and the *Arnee* or *Urna*, are so strikingly alike, as often to be distinguished with difficulty, when seen at a little distance. The only perceptible differences, when compared, consist in the superior size of the *urna*, the greater comparative dimensions of its horns, and in its being a shade darker than the domestic buffalo. Both animals carry their heads with the nose projecting; the horns of both are transversely wrinkled, flattened on the side in the plane of the os frontis, and recline toward the shoulders, in the usual motions of the animal. Their legs indicate great strength, have the metatarsus and metacarpus short and thick, the articulations large, and more bulging than in the domestic ox. The urna, like the buffalo, has large, lowering, fierce eyes; a more convex forehead than the ox; a small but distinct dewlap; and a black skin, thinly covered with blackish hair. In short, the urna is scarcely to be distinguished from the domestic buffalo of India, except, as before remarked, by its superior size, and vast horns; but these circumstances no more entitle it to rank as a distinct species, than the size of the Lancashire cow divides it from the diminutive species of the Highlands of Scotland; or than the enormous horns of the Galla oxen, so well figured in Salt's Abyssinian Travels, separate them from the race of our domestic cattle. The natives of India are so perfectly satisfied of the

identity of the buffalo and urna, that the latter is usually called the *Wild Buffalo.*

The case is very different with the *Gour*. This animal is considered by the Indians, as of a species totally distinct from either; and is even said to have a strong antipathy for the urna, which it will not suffer to intrude upon its domains; while the differences in its external form and habits, are such as to mark it of a different species; and, as it is well termed in a MS. journal of a hunting party, now before me, as "the giant of the bovine race." A perusal of this MS., and the personal explanations of Captain Rogers, who was on that excursion, undertaken for the purpose of hunting the *gour*, have led me to believe that the animal is unknown to our systematic naturalists; and I am not aware that any detailed account of it has ever been published.

The only animal with which it appears to have affinity is the *Gaijal*, or *Bos Gavæus*, described in Mr Colebrooke's communication, in the Asiatic Researches *. That animal is said to exist, both wild and domestic, in the hilly countries of Upper India; to have a high dorsal ridge, somewhat similar to what we shall immediately find in the gour: but the very different form of its head, the presence of a *distinct dewlap*, and the general habit of the gaijal, appear sufficient to distinguish it from the gour; and Captain Rogers assures me, that neither the descriptions in Mr Colebrooke's communication, nor the figure of the gaijal that accompanies them, have any greater resemblance to the gour, than that general one which subsists between all the animals of this genus.

The *gour*, according to that gentleman, occurs in several mountainous parts of Central India, but is chiefly found in *Myn Pât* †, or *Mine Paut*, a high insulated mountain, with a tabular summit, in the province of Sergojah, in South Bahar.

This table-land is about 36 miles in length, by 24 or 25 miles in medial breadth, and rises above the neighbouring plains probably 2000 feet. The sides of the mountain slope with considerable steepness, and are furrowed by streams that water nar-

* Vol. viii. Art. 8.

† Pât or Paut, in Hindûstanee, signifies *table-land.*

row valleys, the verdant banks of which are the favourite haunts of gours. On being disturbed, they retreat into the thick jungles of *saul-trees* *, which cover the sides of the whole range. The SE. side of the mountain presents an extensive mural precipice, from 20 to 40 feet high. The rugged slopes at its foot are covered by impenetrable green jungle, and abound with dens, formed of fallen blocks of rock, the suitable retreats of tigers, bears, and hyænas. The western slopes are less rugged; but the soil is parched, and the forests seem withered by excess of heat. The summit of the mountain presents a mixture of open lawns and woods. There were once 25 villages on Myn Pât; but these have been long deserted, on account of the number and ferocity of the beasts of prey. On this mountain, however, the gour maintains his seat. The Indians assert, that even the tiger has no chance in combat with the full grown gour, though he may occasionally succeed in carrying off an unprotected calf. The wild buffalo abounds in the plains below the mountain; but he so much dreads the gour, according to the natives, that he rarely attempts to invade its haunts; and the hunting party only met with three or four urnas on the mountain. The forests which shield the gour abound, however, with hog-deer †, saumurs ‡, and porcupines §.

According to the Indian mode of hunting, the jungles were beaten by numerous parties of natives, and the European sportsmen, well armed with rifles, took their stations in places where the roused up game were likely to pass. Several gours were shot. One, struck by Captain Rogers and his companion, "made for the jungle, was pursued, and fell, after receiving six or seven balls." Another, when wounded, turned on his assailant, shook his head in token of defiance, and was fortunately shot dead, by a large rifle ball, which penetrated the brain, as he was rushing forward to attack the adventurous hunter.

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* A common forest-tree of this part of India, which is used in building, on account of the durability of its reddish-coloured wood.

† *Cervus porcinus.*

‡ *Cervus Elephas*, of a large size. Its Sanscrit name is *Sambur.*

§ *Histrix cristata.*

The size of the gour is its most striking peculiarity. The dimensions of those killed on that excursion were unfortunately not noted; but the following measurement of a gour, not fully grown, which was killed on another occasion, by one of these gentlemen, will shew the enormous bulk of the animal.

	Feet.	Inches.
Height from the hoof to the withers, - -	5	11⅝
——— from the withers to the sternum, -	3	6
Extreme length from the nose to the end of the tail,	11	11¾

Captain Rogers assures me, that several of the gours killed on Myn Pât were considerably larger than the dimensions here assigned.

The form of the gour is not so lengthened as that of the urna. Its back is strongly arched, so as to form a pretty uniform curve, from the nose to the origin of the tail, when the animal stands still. This appearance is partly owing to the curved form of the nose and forehead, and still more to a remarkable ridge, of no great thickness, which rises six or seven inches above the general line of the back, from the last of the cervical to beyond the middle of the dorsal vertebræ, from which it gradually is lost in the outline of the back. This peculiarity proceeds from an unusual elongation of the spinous processes of the dorsal column. It was very conspicuous in the gours of all ages, although they were loaded with fat; and has no resemblance to the *bunch* which is found on some of the domestic cattle of India. It bears some resemblance certainly to the ridge described as existing on the gaijal; but the gour is said to be distinguished from that animal by the remarkable peculiarity of the total want of a *dewlap*. Neither male nor female gour, of any age, has the slightest trace of this appendage, which is found in every other known animal of this genus.

The colour of the gour is a very deep brownish-black, almost approaching to bluish black, except a tuft of curling dirty white hair between the horns, and rings of the same colour just over the hoofs. "The hair over the skin is extremely short and sleek, and has somewhat of the *oily* appearance of a fresh sealskin."

The character of the head differs little from that of the domestic bull, excepting that the outline of the face is more curved, the os frontis more solid and projecting. The horns are short, thick at the base, considerably curved towards the tip, slightly compressed on one side, and, in the natural state, are rough. They are, however, capable of a good polish, when they are of a horn-grey colour, with black solid tips. A pair in my possession measures 1 foot 11 inches along their convex sides, 1 foot from the centre of the base to the tip, in a straight line, and 1 foot in their widest circumference; but as they are cut and polished, a portion of their length and thickness has been lost. They are of a very dense substance, as their weight indicates, for, even in their dressed state, the pair weighed 5 lb. 11 oz. avoirdupois. The eye is smaller than in the domestic ox; it is of a light blue colour; and, from the projection of the eye-brow, has somewhat of a fierce expression, yet is milder than that of the urna.

The limbs of the gour have more of the form of the deer than any other of the bovine genus. This is particularly observable in the acuteness of the angle formed by the tibia and tarsus, and in the slenderness of the lower part of the legs. They give the idea, however, of great strength, combined with fleetness; and the animal was observed to *canter* with great velocity. The form, too, of the hoof is "longer, neater, and stronger than in the ox: and the whole foot appears to have greater flexibility."

The extremity of the tail was bushy; but its comparative length was not noted by my informant.

The gour was not heard to utter any cry until wounded, when it emitted a short bellow, which may be best imitated by the syllables *Ugh—ugh.*

The natives informed one of the sportsmen, that the gour will not live in a state of captivity. Even when taken very young, the calf soon droops and dies. The period of gestation for the females is twelve months; and they bring forth usually in the month of August. The cow yields a large quantity of milk, which the Indians aver to be occasionally so rich, as to cause the death of the calf. The bull calf of the first year is,

by the natives, called *Purórah*; the female calf is named *Parēeah:* and, when full grown, the cow is called *Goùrin.*

Gours associate in herds, consisting usually of from ten to twenty animals. So numerous are they on Myn Pât, that, in one day, the hunting party computed that not less than 80 gours had passed through the stations occupied by the sportsmen.

The gours browze on the leaves and tender shoots of trees and shrubs, and also graze on the banks of the streams. During the cold season, they remain concealed in the *saul* forests, but in hot weather, come out to feed in the green valleys and lawns which occur on the mountain of Myn Pât. They shewed no disposition to wallow in mires or swamps, like the buffalo; a habit, indeed, which the sleekness of their skins renders not at all probable.

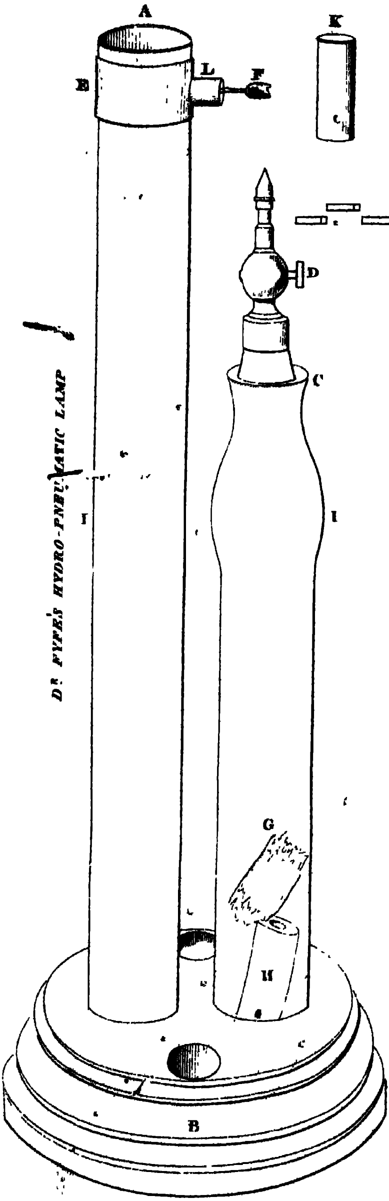
Such are the particulars which I have been able to collect respecting the *Gour*. Whether they are sufficient to characterize it as a distinct species of *Bos*, I shall not pretend to decide. The size, form, horns, and, above all, the want of a dewlap, incline me to this conclusion; but we may soon hope to have the point ascertained beyond dispute, by the transmission to England of further observations, and of a correct figure of the animal, by some of our enterprising countrymen in India *.

PLATE XI

Fig. A. Outline of the base of one of the polished horns of the Gour.

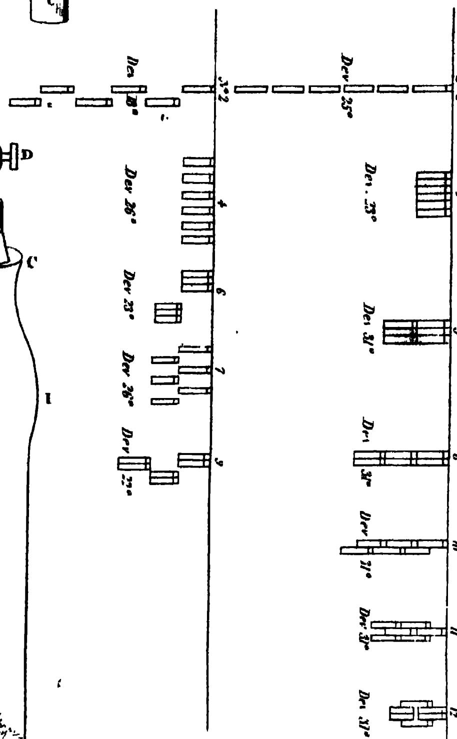
Fig. B. Sketch of the same horn, with its convex side towards the eye.

* In the 9th volume of the *Mem. du Mus. d'Hist. Nat.*, Geoffroy St Hilaire gives some particulars in regard to the natural history of the Gour. He considers the remarkable *dorsal ridge* as produced by bones superadded to its dorsal spinous processes, and that this arrangement is analogous to the rays in the fins of fishes. —Edit.

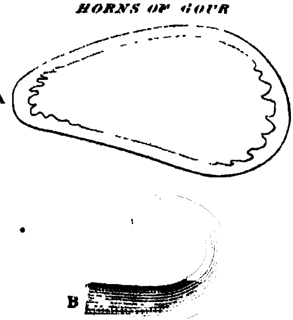


Dr. FYFE'S HYDRO-PNEUMATIC LAMP

SCORESBY'S MAGNETICAL EXPERIMENTS



HORNS OF GOUR



Published by A. Constable & Co. Edin^r 1824

W. H. Lizars sculp^t

ART. XVII.—*Description of a Hydro-Pneumatic Lamp.* By ANDREW FYFE, M. D. F. R. S. E. Lecturer on Chemistry, Edinburgh.

THE discovery of Professor Döbereiner, of the ignition of spongy platinum by hydrogen, naturally led to its application as a means of affording an instantaneous light-giving machine. Different instruments have accordingly been constructed for this purpose; the first of which was that some time ago recommended by Gay Lussac, for keeping a store of hydrogen, and improved by Mr Garden of London. It is, however, expensive, and not easily managed by those not accustomed to the use of chemical apparatus.

Another has been invented by Mr Adie, which, though much less complicated, may, by many, be considered also as too expensive. The one I have employed for some time past, combines the advantages of simplicity, cheapness, and facility of management, while it answers the purpose equally well with those above mentioned.

It consists merely of a bent glass-tube ABC (Plate XI.), the internal diameter of which is nearly an inch. It is open at both ends; fixed into a wooden stand B. The short limb C, is 5, and the long one A, 8 inches in length. To the mouth C there is ground a glass-tube, to which is fixed a stop-cock D. At E there is a brass ring fitted closely to the tube, and from which there proceeds a piece of brass, to which the spongy platinum F is fixed by *very fine* wire.

As the platinum loses its power of ignition by exposure to air, or rather requires a large supply of hydrogen, I have it covered with a cap, represented by K, and which is ground accurately on the cylinder L. When the lamp is required for use, a piece of zinc is put into the short limb as at G, and which is prevented from coming nearer than about an inch from the bending, by a tube of glass as at H. Diluted sulphuric acid is then poured in, so as to fill, as far up as I, after which the stopper and stop-cock are introduced. By the action of the acid on the metal, hydrogen is generated, which fills the short limb, and raises the fluid in the opposite one: and the production of gas

ceasing when the acid gets below the zinc, there is thus always a supply of gas subjected to the pressure of a column of fluid of from six to seven inches. When the stop-cock, therefore, is opened, the hydrogen is propelled against the platinum; the fluid falls into the short limb; and as the zinc is thus again surrounded by acid, more gas is generated to serve for the next time. The distance of the platinum depends on the size of the bore of the stop-cock; but as the ring E is moveable, it can be easily adjusted.

The apparatus described contains only about a cubic inch of gas; but I find it is sufficient for affording a light, for, though it does not ignite the platinum long enough to kindle the gas, yet there is sufficient heat to set fire to a sulphuric match. The moment, then, that the platinum becomes red-hot, a small sulphuric match must be applied to it. The only circumstance to be attended to, is to allow the match to remain ignited for a few seconds, with a view of driving off any sulphur that may be left adhering, and which prevents the ignition when the gas is again impelled on it. I would therefore recommend, that, each time it is used, the flame of the match be kept at it for a short time.

I am aware that many improvements on this lamp might be suggested, and which, it must be admitted, would make it more complete, but they detract from one of its greatest qualifications, cheapness; the lamp, as described, being purchased for about ten shillings.

ART. XVIII.—*Account of several new Species of Combretum, Chailletea, Clerodendrum, Gomphia, Modecia, Mussænda, Justicia, Brillantaisia, Parinarium and Anthocliesta, from Sierra Leone.* By Mr GEORGE DON, A. L. S.

IN a voyage undertaken by me in 1822 and 1823, for the purpose of collecting seeds and plants for the Horticultural Society of London, I visited the western coast of Africa, the Brazils, some of the West India islands, and New York. Perhaps the most interesting spot I visited, was the colony of Sierra Leone, where I remained from the 18th of February 1822 till the 11th

of April following. Other parts of the western coast of Africa were also interesting, particularly the island of St Thomas. The number of rare and splendid plants found in that quarter, and remaining yet unpublished, induces me to give descriptions of some new species belonging to those genera which are most likely to arrest attention, either for their beauty, or for some particular qualities belonging to them. They are all of them highly deserving of being cultivated in our stoves; and several of them I have been so fortunate as to introduce into this country in a living state. I have marked with an asterisk such of them as are to be seen growing in the garden of the Horticultural Society at Chiswick. All those enumerated in the following pages are *new*, or now described for the first time; with the exception of *Gomphia reticulata*, *Ochna multiflora* and *Justicia hispida*. Besides those here described, many others deserve to be noticed for their elegance or for their showy flowers, most of which are hitherto unnoticed in botanical works. These may form the subject of another communication.

It may perhaps be interesting, in the mean time, to mention the names of a few of the most likely to attract the attention of persons visiting the colony. They are as follows:—**Clematis grandiflora*, DeCandolle; **Cratæva fragrans*, Bot. Mag.; **Ipomœa splendens*, new, like *I. paniculata*; **Ipomœa involucrata*, Beauvois; *Banisteria Leona*, and a new genus nearly related, with alternate leaves †; **Erythrophleum*, Afz. the Red Water, or Gregre Tree of the Negroes; *Loranthus*, two species; **Lophira alata*, the Scrubby Oak of the colonists, with its long, leathery, pale green leaves, and panicles of white flowers, very conspicuous; **Osbeckia grandiflora*, Afz. herbaceous, and roots tuberous, flowers large, blue; *Rondeletia febrifuga*, Afz.; **Smeathmannia lævigata* and *pubescens*, Solan.‡; *Spathodea campanulata* and *lævis*, Beauv.; **Tetracera potatoria*, Afz. the Water-Tree of the colonists; *Gardenia coccinea*, new, flowers the size of those of *Vinca rosea*; *Gardenia pulchella*, flowers white, throat yellow, the size of the preceding, with many

† See Mr Brown on the Congo Plants.

‡ See Mr Brown in the *Linn. Trans.*

other species; * *Cassia conspicua*, new, like *C. fistula*, the Monkey Drum-stick Tree of the colonists; **Arum compressum*, new, bulb large, round, and flat; **Aletris*, several species; *Pavetta*, two or three species; *Strophanthus sarmentosus*; **Bletia Guinense*, new, like *B. verecunda*, but larger; *Hæmanthus multiflorus*, with many others. All the fruit-trees of Sierra Leone are likewise interesting, but are now mentioned in the 4th Part of the 5th volume of the London Horticultural Society's Transactions, where there is a figure of **Sarcocephalus esculentus*, the Sierra Leone Peach.

Professor Afzelius of the University of Upsal, who resided in the colony of Sierra Leone for several years, formed an extensive collection in every department of natural history. It is much to be regretted, that so distinguished a naturalist should not have given his discoveries to the world soon after his return to Europe. Among his collection of plants in the Lambertian Herbarium, are specimens of all those noticed in this paper. The examination of this collection proved of great benefit to me.

COMBRETUM. *Linn. et Schreb.*

1. *C. *comosum*; scandens; ramulis pubescentibus, foliis oppositis, oblongis acutis integerrimis, adultis glabris, basi subcordatis, breve petiolatis; racemis paniculatis, brachiatis, spicis elongatis confertifloris; bracteis lanceolatis acutis.

Hab. Ubique circa Sierra Leone.

Panicula composita, spicis numerosis elongatis oppositis et verticillatis. *Flores* comosi.

The *C. comosum*, is a beautiful climbing shrub, and attaches itself to other shrubs which it grows near: if growing in a place clear of other plants, it supports itself, and in these cases assumes a conical form. The younger branches are pubescent. The leaves are opposite, large, oblong, pointed, entire, smooth, and at the base somewhat cordate, upon short footstalks. The panicles are brachiate, composed of numerous, elongated, opposite or verticillate spikes. The flowers are crowded or tufted, about twice the size of those of *C. purpureum*; of a deep scarlet colour. The seed-vessels, before they be perfectly ripe, are of a pale red, which makes a striking contrast. This plant is very common in the low lands as well as in the mountains of Sierra

Leone. The profusion and brilliancy of the blossoms, often attract the attention of strangers.

2. C. *intermedium;* scandens; ramulis pubescentibus; foliis oppositis vel verticillatis, oblongis obtusis integerrimis, adultis glabris, mucronatis, basi subcordatis, breve petiolatis; panicula brachiata; spicis elongatis, floribus densissimis; bracteis ovatis acutis.
HAB. Ad rivulorum margines in Sierra Leone.
Habitus præcedentis, sed folia sunt obtusa, et flores duplo majores et densiores.

This is a beautiful climbing shrub, and is nearly related to the preceding, from which, however, it is very distinct, the flowers being nearly double the size, and more crowded. The panicles are brachiate, composed of numerous, opposite, but more commonly verticillate spikes. The leaves are often in whorls, oblong-obtuse, smooth, and entire, standing on short footstalks. This shrub is of rather rare occurrence, and always grows by the sides of rivulets in the low lands of Sierra Leone. I did not see more than three or four plants during my stay in the colony. This shrub is more straggling than *C. comosum.*

3. C. *paniculatum;* scandens; paniculâ diffusâ.
HAB. In Sierra Leone.
Flores non vidi.

This is a tall climbing shrub, sometimes extending to the tops of the loftiest trees. The flowers I did not see; but, from the habit of the plant, I should expect them to be very different from any of the preceding. The seeds grow in loose, scattered panicles, quite different from the other species. This plant is found growing in small islands opposite to the settlement of York, about eighteen miles from Freetown; and no where else have I seen it.

4. C. *spinosum;* erectum ramosum; ramis spinescentibus; foliis oppositis oblongis integerrimis, adultis glabris, petiolatis; racemis paniculatis, axillaribus terminalibusque.
HAB. In fruticetis depressis circa Sierra Leone.
Flores magnitudine C. *purpurei;* rami seniores spinosi.

C. *spinosum* is an upright, branching shrub, with the older branches spiny. The leaves are oblong-entire, smooth, standing on longer footstalks than the rest. The racemes are paniculated, axillary and terminal. Flowers scattered, of a deep

purple colour, about the size of those of C. *purpureum*. This plant grows on the road-side leading to Congo Town. I also observed it on King Tom's Point, a little distance from Freetown, but by no means common.

5. *C. *grandiflorum*; humile, subscandens; foliis oppositis, cordato-oblongis, mucronatis integerrimis, utrinque ramulisque hirsutis, breve petiolatis; racemis secundis axillaribus terminalibusque.

Hab. In locis depressis ad Sierra Leone.

Flores magni, coccinei. *Antheræ* luteæ.

This is a low, somewhat climbing shrub. The leaves are opposite, cordate, oblong, mucronate, entire, hairy as well as the young branches, upon short petiols. Racemes axillary and terminal. The flowers are very large, and secund, of a deep vermilion colour. It grows plentifully near Freetown, and on the road to Congo Town. The flowers being so large, have at first sight the appearance of those of a species of *Ipomœa*, being as large as in *Ipomœa Quamoclit*.

6. *C. *leucophyllum*; erectum, ramosum; foliis oppositis oblongis obtusis mucronatis, utrinque ramulisque hirsutis, integerrimis breve petiolatis, junioribus niveis; racemis umbellatis terminalibus abbreviatis.

Hab. In locis depressis ad Sierra Leone.

Flores fastigiati, umbellati. *Antheræ* nigræ. *Folia juniora* nivea.

This is a most beautiful upright shrub. The leaves are opposite, oblong, obtuse, pointed, hairy as well as the younger branches. The floral leaves are of a whitish-cream colour. The flowers are scarlet, with black anthers, growing in umbel-like racemes at the extremity of the branches. The C. *leucophyllum* grows near Congo Town, and also in King Tom's Point, a little distance from Freetown. The scarlet blossoms, with the white floral leaves and black anthers, have a very gaudy appearance.

7. C. *tomentosum*; ramosum, tomentosum; foliis oppositis oblongis rotundatis mucronatis integris, breve petiolatis; racemis axillaribus confertifloris.

Hab. In fruticetis ad Sierra Leone.

Frutex scandens? *Flores* parvi, albi.

The C. *tomentosum* grows in shrubby places both in the low lands and in the mountains of Sierra Leone. The whole shrub is woolly. The leaves are opposite, oblong, round, mucronate,

upon short footstalks. The flowers are small and white, in crowded, elongated, axillary racemes.

8. C. *micranthum*; erectum, ramosum; foliis ovalibus acutis integerrimis, petiolis brevibus ramulisque pubescentibus; racemis brevibus tenuibus axillaribus.

HAB. In fruticetis depressis ad Sierra Leone.

Flores parvi, rubri.

This shrub is upright and branching. The leaves are opposite, oval-acute. The petiols are short and pubescent, as well as the younger branches. The flowers are small and red, growing in short, slender, axillary clusters, and sometimes the clusters are so numerous and crowded at the apex of some of the branches, as to assume the appearance of crowded panicles. It grows in shrubby places in the low lands, and is sometimes to be found in the mountains of Sierra Leone.

9. C. *herbaceum*, humile; ramis calycibusque pubescentibus; foliis alternis lanceolatis mucronatis integris, subtus sericeis, breve petiolatis; pedunculis axillaribus, paucifloris, racemiformibus.

HAB. In locis depressis ad Sierra Leone.

Radix lignosa. *Caulis* simplex, erectus, herbaceus, pedalis et ultra. *Flores* parvi, albi.

C. *herbaceum* is a small herbaceous plant, about a foot high. The stems are simple and pubescent, as well as the footstalks, which are short. The leaves are alternate, lanceolate, mucronate, entire, silvery underneath. The peduncles are axillary, bearing from seven to eight flowers, racemose. The flowers are small and white. This singular little plant grows abundantly in clear low lands near Freetown. It has little of the general appearance of *Combretum*, but has certainly all the essential characters of that genus.

10. C. *sericeum*; suffruticosum, erectum, calycibusque pubescens; foliis alternis lanceolatis oblongis integris, subtus sericeis, breve petiolatis; racemis axillaribus, confertifloris.

HAB. In locis depressis ad Sierra Leone.

Flores parvi, albi.—Obs. C. *herbaceo* affine.

C. *sericeum* is nearly related to C. *herbaceum*, but the plant is more shrubby. The leaves are not pointed. The flowers grow in axillary clusters, are very much crowded, and are rather smaller than those of C. *herbaceum*. The plant is upright, pubescent, as well as the peduncle and calyx. The leaves

are oblong-lanceolate, silvery underneath. The flowers are small, whitish, appearing in crowded axillary clusters. This plant grows in the low lands of Sierra Leone.

CHAILLETEA. *De Candolle*, Ann. de Mus.

1. C. *toxicaria*; foliis alternis oblongo-lanceolatis acuminatis, margine undulatis, integris glabris coriaceis, breve petiolatis; racemis paniculatis axillaribus terminalibusque; drupa ovata, pubescente.
 Hab. In montibus apud Sierra Leone.
 Frutex 10 pedalis et ultra, ramosissima. *Flores* parvi, albi.

The English name of *Chailletea toxicaria* is Ratbane. There is a deadly poison prepared from the kernel of the fruit by the Negroes, which they use for the purpose of poisoning rats, whence this name. The shrub is from 8 to 12 feet high, branching much. The leaves are alternate, oblong-lanceolate, coriaceous, pointed and entire, smooth, with the margin waved, upon short footstalks. Panicles terminal and axillary. Flowers small, white. Fruit ovate, pubescent, about the size of a pigeon's egg. This plants grows plentifully in the mountainous parts of Sierra Leone.

2. C. *toxicaria* var. *compressa*; fructu subrotundo, subcompresso, pubescente.
 Hab. In montibus apud Sierra Leone.
 Frutex 10 pedalis, et ultra, ramosa.

This is in every respect the same as C. *toxicaria*, but the whole shrub is much larger in all its parts; the fruit rounder and rather flattened. It grows mixed with the former in the mountains, but is not so plentiful. The kernel of the fruit possesses the same poisonous qualities as that of C. *toxicaria*.

3. C. *erecta*; ramis elongatis, erectis; foliis alternis lanceolatis oblongis obtusis acuminatis integris glabris coriaceis rigidis; floribus axillaribus; drupis ovatis subrotundis pubescentibus.
 Hab. In montibus apud Sierra Leone.
 Frutex 10 pedalis, sæpe ultra; ramis elongatis erectis.

The kernel of the fruit of this species possesses the same poisonous qualities as that of C. *toxicaria*. It is found very plentifully in the mountains of Sierra Leone. The shrub is from 8 to 12 feet high. The branches elongated. The leaves are alternate, oblong-lanceolate, acuminate, smooth, coriaceous. Flowers axillary. Fruit about twice the size of that of C. *toxicaria*, and less pubescent.

CLERODENDRUM. *Linn.*

1. C. *hirsutum;* scandens, pilosum; foliis oppositis oblongis acutis integris sæpe subcordatis, utrinque hirsutis, longe petiolatis; panicula dichotoma, patente terminali.
   HAB. In montibus apud Sierra Leone.
   Flores albi. *Filamenti* purpurei. *Antheræ* luteæ.

The *Clerodendrum hirsutum* is a small climbing, hairy shrub, from 2 to 3 feet high. The leaves are opposite, oblong, acute, entire, hairy, and often at the base somewhat cordate, upon long petiols. The panicles are terminal, dichotomous, and spreading. The flowers are white, with purple filaments and yellow anthers. This shrub grows plentifully in the mountains of Sierra Leone.

2. C. *simplex;* erectum, pilosum; foliis oppositis oblongis acuminatis integris, longe petiolatis; panicula dichotoma patula terminali.
   HAB. In montosis prope Sierra Leone.
   Frutex 1½ pedalis. *Flores* albi. *Filamenti* purpurei. *Antheræ* luteæ.

This is a small upright plant, about 1½ foot high. The stems are simple and hairy. The leaves are opposite, oblong, pointed, entire, upon long petiols. The panicles are dichotomous, spreading, and terminal. Flowers white, with purple filaments and yellow anthers. This beautiful little species grows in the mountainous parts of Sierra Leone, but is rather rare.

3. C. *splendens;* scandens, glabrum; foliis oppositis ovatis oblongis acuminatis integris, breve petiolatis; panicula dichotoma patente terminali.
   HAB. In montibus apud Sierra Leone.
   Flores splendide coccinei. *Filamentæ* luteæ. *Antheræ* subcœruleæ.

C. *splendens* is one of the greatest ornaments in the low bushy parts of the mountains of Sierra Leone. I have also observed it in the low lands, but by no means common. The shrub is climbing and smooth. The leaves are opposite, ovate-oblong, pointed, entire, at the base often somewhat cordate, and smooth, dark green; the petiols are short. The panicles are dichotomous, spreading, many-flowered, growing at the extremity of the branches. The flowers are of a deep scarlet colour, with yellow filaments, and bluish anthers.

4. C. *aurantium;* scandens, glabrum; foliis oppositis oblongis, acuminatis mucronatis integris, breve petiolatis; panicula dichotoma confertiflora, terminali lateralique.
   HAB. In montibus ad Sierra Leona.
   Flores aurantii. *Filamentæ* luteæ. *Antheræ* subcœruleæ.

This shrub is nearly related to the preceding. It is climbing and smooth. The leaves are opposite, pointed, entire. The petiols are short. The panicles dichotomous, crowded, many-flowered, terminal or lateral. Flowers orange-coloured. Filaments yellow, anthers bluish. This plant grows on the mountains of Sierra Leone.

5. C. *multiflorum*; assurgens, glabrum; foliis oppositis ovato-lanceolatis acuminatis, interne rugosis, longe petiolatis; panicula confertiflora terminali.

HAB. In Sierra Leone monte, Leicester Mountain Anglice dicto.

Flores parvi, albi. *Frutex* 12 pedalis.

C. *multiflorum* is rather an upright plant. The leaves are rather rugose, smooth, ovate-lanceolate, pointed, entire, upon long petiols. The panicles are many-flowered, crowded, terminal. The flowers are small and white. This shrub grows on Leicester Mountain, and no where else have I seen it.

GOMPHIA. *Schreb. Gen.*

1. G. *reticulata* *; foliis lanceolatis oblongis glabris serratis acutis reticulatis, breve petiolatis; panicula diffusa, terminali.

HAB. In locis depressis ubique circa Sierra Leone.

Frutex 3 vel 4 pedalis, ramosa. *Corolla* lutea.

The *Gomphia reticulata* is very common in the low lands, and also in the mountains, of Sierra Leone. It is a small branching shrub, about 3 or 4 feet high. The leaves are oblong-lanceolate, pointed, smooth, at the margin serrated, upon short petiols, the veins reticulated. The flowers are yellow, and in terminal, spreading, branching panicles; the peduncles are short, and often three together. This plant has also been found plentifully in the Kingdoms of Oware and Benin, by the Baron de Beauvois, who has given an excellent figure of it in his *Flore d'Oware et Benin*.

2. G. *congesta*; foliis oblongis obtusis, æqualiter serratis, breve petiolatis; panicula terminali, diffusa, confertiflora.

HAB. In locis depressis circa Sierra Leone.

Frutex 3 vel 4 pedalis, ramosa. *Corolla* lutea.

This shrub is nearly related to the preceding, but differs from it in having the leaves oblong-obtuse, and the flowers more

* Beauvois in the *Flore d'Oware*.

crowded. This new species grows in low places not far from Freetown, but is rather rare.

3. G. *integrifolia;* foliis lanceolatis oblongis acuminatis integerrimis glabris breve petiolatis; panicula terminali, diffusa.
 HAB. In maritimis juxta Promontorium Shilling Anglice dictum.
 Frutex 3 vel 4 pedalis, ramosa. *Corolla* lutea.

G. *integrifolia* is a beautiful new species. The leaves are oblong-lanceolate, acuminate, smooth and entire, upon short petiols. The panicles are spreading and terminal. The peduncles are short, and often three together. The flowers are yellow. The shrub is very slender. It grows by the sea-side near Cape Shilling.

OCHNA. *Linn.*

1. O. *multiflora;* foliis lanceolatis oblongis acuminatis integris repandis glabris, breve petiolatis; racemis simpliciter lateralibus, elongatis.
 HAB. In locis depressis prope Sierra Leone.
 Frutex 5 vel 6 pedalis, erecta. *Corolla* lutea.

This species appears to have been first discovered by Smithman, and is described by M. De Candolle in the *Ann. du Mus.* vol. xvii., from his specimen. The shrub is about 5 or 6 feet high, erect. The leaves are oblong-lanceolate, pointed, smooth, entire, and waved at the margin, upon short petiols. The flowers are yellow, in simple, elongated, lateral racemes. The peduncles are short. I observed this shrub near Cape Sierra Leone. My specimen differs from the figure in the *Ann. du Mus.* in the leaves being much longer and narrower, and waved on the margin.

(To be concluded in next Number.)

ART. XIX.—*Conjectures regarding the Original Form of the Pyrenees.* By M. J. DE CHARPENTIER, Directeur des Mines de Canton de Vaud, &c.*

THE examination of the structure of the Pyrenees, and especially the observations which may be made upon the relations

* These conjectures regarding the Pyrenees are extracted from Charpentier's "*Essai sur la Constitution Geognostique des Pyrenees,*" a work of distinguished merit, which was crowned by the Royal Institute of France.

between the disposition of the rocks and the external form of the chain which they compose, furnish the geologist with ample materials for establishing interesting conjectures regarding the original form of these mountains, and the revolutions which, by modifying and breaking down this form, have successively given rise to that under which they now present themselves.

We shall attempt briefly to point out these conjectures; and, for this purpose, we shall recapitulate the principal facts which the arrangement and disposition of the rocks have exhibited in the Pyrenees.

" We have seen, that the different formations are disposed in bands parallel to each other, and parallel to the general direction of the Pyrenees; that the granite forms only a single band, or, speaking more correctly, a chain or series of protuberances; that each of the other formations constitutes in general two bands, one of which is situated to the north, the other to the south of the granitic chain, resting upon it in the order of their relative antiquity; that many of these granitic protuberances are separated from one another by valleys, while others, on the contrary, are, as it were, agglutinated by rocks of later origin, which have filled up the spaces or vacuities by which they were formerly separated; and, lastly, that it is commonly in the spaces which exist between two great protuberances that we observe the bands that occur to the south of the granitic chain, touching and mingling with those which occur to the north.

These facts entitle us to presume, that the granitic formation, comprising that of mica-slate and primitive limestone, formed originally an uninterrupted chain, or rather an elongated line, having a direction from south-east to north-west, and being of a height, whether absolute or relative, much greater than at the present day; that at a period anterior to the formation of the other rocks which recline upon it, this granitic chain has undergone degradations caused by a power (perhaps currents of water) which, acting horizontally from south to north, or from north to south, has broken its ridge in many parts, scooped it out to a great depth, and changed it into a series of more or less isolated eminences; that the rocks formed after this revolution have been applied on each side against this central granitic chain, have filled up its deepest hollows, and have even covered its

lowest protuberances; and that, lastly, immediately after this revolution, the ridge of the primitive formation was, without doubt, at the same time that of the whole chain of the Pyrenees.

Now, as we observe at the present day, that the ridge of the Pyrenees, with the exception of a small number of places, is no longer the ridge of the granitic chain, which is found removed at some distance to the north; but that this geographic ridge is composed of more modern rocks, which generally surpass the primitive formation in height, we are naturally led to presume that the Pyrenees have undergone a second very considerable degradation.

The disposition of the rocks, and the external form of the mountains, appear to determine the period of this revolution. It is probable that it has taken place after the formation of the transition deposite, and before the excavation of the presently existing valleys, and consequently before the deposition of the trap formation, which, as we shall see in the sequel, appears to be of a very late origin.

Observation tends to induce a presumption, that this degradation has principally attacked the ridge then existing, and all the northern aspect of the chain.

We shall represent by a diagram the results which have given rise to this supposition.

Fig. 9. Pl. IX., represents the vertical and transverse section ABC of the Pyrenees in the direction of their breadth, such as we presume it to have been before these mountains underwent the degradation of which we have been speaking. We see in this section the two declivities AB and AD of equal size; the granite occupying the centre, and forming the ridge of the chain; the transition formation, and the secondary formation, distributed in nearly equal quantities, upon the south and north sides, resting upon the granite.

Let us now suppose that all the portion of these mountains situated between A, B, and C, has been destroyed by the effect of some power acting from north to south, in such a manner that there remains only the part situated between C, B, and D.

The necessary consequence of this degradation would be a considerable change in the external form of the whole chain of

mountains, and especially in the disposition and distribution of the rocks with relation to the external form of the chain; in short, this revolution would produce a multitude of results and accidents which are observed in the Pyrenees, and of which we shall recapitulate the chief.

There would result from the destruction of all the parts situted between A, B, and C, 1st, That the ridge would be lowered; and, further, that its position would be removed more to the south, and that consequently the northern aspect B, C, would become longer and more sloping than the southern one BD.

2dly, That the granite, including the other primitive rocks, would no longer form the ridge of the central chain, to the north of which it would occur at a short distance.

3dly, That the southern bands of the secondary and transition formations would obtain a height which would in general surpass that of the granite, and that of all the other rocks situated to the north of the primitive formation.

4thly, That these two southern bands would, in general, form the ridge of the whole system.

5thly, That the transition formation would be much more diffused, or, at least, would appear to a much greater extent, upon the north side than upon the southern declivity.

6thly, That the secondary formation would occupy all the southern declivity, while, on the northern side, it would only form the low mountains at the foot of the chain.

We here see how well the necessary results of the supposition which we have admitted, accord with the actual phenomena.

Several other observations would further lead us to presume, that, independently of the great revolution of which we have been speaking, the northern part of the Pyrenees must have undergone, previously to the formation of the present valleys, a new degradation of considerable extent; such, for example, are the generally softer and more rounded forms of the northern, compared with the southern, mountains; the more considerable number of basins in the French valleys than in those of the Spanish side; and the immense deposites of transported rocks, of which the soil of the plains which extend from the north side of the Pyrenees is formed.

ART. XX.—*Magnetical Experiments, designed to illustrate the Manner of the Existence of the Magnetical Principle in Ferruginous Bodies, and the Mode of its Development.* By WILLIAM SCORESBY *jun.* F. R. S. Lond. & Edin. M. W. S. &c.

THE phenomena of magnetism are so curious, and the principle itself has become of so much importance in science, that any thing which throws light upon the manner of action of this wonderful agent, may not be unworthy of publicity. The experiments that I have to mention, were suggested by an investigation of the manner of the existence of the magnetic principle in ferruginous substances. They are so simple, that I can hardly suppose them to be entirely new, though they were new to me; yet I venture to describe them, because of the obvious illustration they afford of several of the phenomena of magnets; and this is the object I have particularly in view in detailing them.

It is a fact that has been rendered sufficiently clear by M. Biot, that the magnetisms of ferruginous bodies are principles residing at all times in surprising quantity in these bodies. Iron, magnetic or not magnetic, has precisely the same quantity of the magnetic matter within it, the magnetism in the one case, being separated and arranged in a peculiar order, and, in the other, neutralised either by confusion of order, or coalescence. It is a fact equally certain, that the magnetic principle, is, by a constitution of the metal, permanently confirmed in every particle, so as not to permit, by any possibility, either of increase or of diminution. A very simple experiment was found to illustrate this. I took a steel-wire of $\frac{1}{8}$th of an inch in diameter, and 10 inches in length. I notched it about $\frac{1}{3}$ through, for separation into six equal portions, and then magnetised it strongly, in a way that prevented the liability to *consecutive points*; that is, so as to give an equal distribution of the magnetic power, to confine the poles to two, and to leave those poles at the extremities of the wire. The iron was then broken at the notch nearest to the north pole, and the attractive force of the fragment, on a small compass placed at the distance of six inches, was a deviation of 19 degrees. The next central por-

tion was then removed, and its attractive force determined, which was 20°; and so on of the rest in succession. The deviation of the needle produced by the north pole of each in succession was, No. 1., 19°; No. 2., 20°; No. 3., 20°; No. 4., 21°; No. 5., 20°½; No. 6., 20°½; and of the south poles, No. 1., 4°; No. 2., 4½°; No. 3. 4½°; No. 4., 4°; No. 5., 4°; and No. 6., 4° *. If, in the magnetising powers, any change in the magnetism of each particle had taken place, it is evident that there must have been a greater attraction of northern polarity in the fragment at the North Pole, than in that at the south; and that in the central fragments, which, before separation, were without attractive force, there must have been less magnetic intensity than in the fragments detached from the extremities, neither of which effects occurred: it is therefore evident, that the magnetisms were equally distributed throughout the bar, even when its condition was such, that its poles exhibited a powerful degree of magnetism, and its centre none.

The manner of the existence of the magnetic principles, therefore, is in each particle; and the phenomenon of the magnetic condition seems to consist of such an arrangement of the principles as may form a *battery* of magnetic particles. Whether this arrangement is effected at the moment of giving the sensible magnetism to the body, by a decomposition of the two magnetisms (two principles being supposed), which were before neutral in each particle by combination; or whether it consists in giving a new arrangement to the magnetic principles, according to the axes of the bar, supposing them to be moveable in the particles; or whether it may consist of a change in the position of the molecules of the iron into the required arrangement, is uncertain; but, in either case, the apparent phenomena must be similar. If the first supposition be the true one, then the magnetic condition is the decomposition of the magnetisms, and their arrangement in a certain order, according to the direction of the axes of the magnet; and the unmagnetic condition, the mutual neutralising of the two principles in each particle. If the second be the true one, then the magnetic condition requires

* The reason why the southern poles had less magnetic force than the northern, has not yet been investigated.

only one arrangement to be produced in the magnetisms; the unmagnetic condition being the disorderly position of the magnetisms in each particle. If the third be the correct one, then the molecules of the iron must be mobile on the application of an external attractive force, and an arrangement of the particles of the iron be necessary to the development of the magnetic condition.

In whatever manner the effect is produced, the following experiments, I conceive, will shew that the magnetic force of any bar is the result of the combined and aggregated magnetisms of all the individual particles, in the same way that the power of the galvanic battery is the result of the aggregate of the electricities of each pair of plates. These experiments, I may premise, were all made with small magnets of $1\frac{1}{2}$ inches long, and $1\frac{1}{10}$ diameter; and their magnetic force in connection, and in various arrangements, was determined by the deviation produced on the compass of the *magnetimeter*, at the distance of six inches from the extremity of the nearest magnet.

1. I combined six of these wires in a regular series of alternate north and south poles in connection, so as to form a straight line, the nearest position being at the distance of 6 inches, and the most remote 15 inches from the centre of the compass. The deviation that resulted was 25°. (Pl. XI. No. 1.) Then, breaking the connection of the series (as in No. 2.), but keeping the magnets the same distance from the compass, the deviation diminished to 18°.

2. I next placed the six magnets in a parallel series, all in contact (No. 3.), and at the distance of 6 inches from the compass, when the deviation was found to be 23°, indicating a force somewhat less than that given by the first series or battery, although five out of six of the magnets were, on an average, $4\frac{1}{2}$ inches nearer to the compass than before, and exerting, consequently, near *thrice* the force they were capable of, when in their more remote position. The wires being afterwards separated, (as in No. 4.), the attraction increased to 26°.

3. Varying the experiment, the six magnets were formed into three parallel series in contact, consisting of two magnets in length (No. 5.), the north poles of one series being in contact with the south poles of the other; the deviation in this case was

31°, and when they were separated in two divisions (as in No. 6.), it diminished to 23°; but increased, on a separation of each magnet (No. 7.), to 26°.

4. The next arrangement was in two parallel lines in contact, of three magnets each (No. 8.), when the deviation was again 31°; and, separated into three portions (as in No. 9.), it diminished to 22°.

5. In like manner, each of the arrangements represented in Pl. XI. No. 10, 11, and 12, gave a deviation also of 31°; but on each magnet being disconnected, it diminished to 25°, 26°, and 28°; and, separated only partially (that is sidewise, but connected lengthwise), the deviation was 29° in No. 11., 30° No. 12., and 31° No. 13.

Now, it is a remarkable result of these simple experiments, that, in the various arrangements that were made, a great acquisition of power above that of the detached power of all the magnets, was invariably obtained, by uniting them in a *magnetical* order,—that is, either with north and south poles, contiguous, and in contact (as in Nos. 1, 5. and 8.); or, with the northern portion of one laid parallel to, and in contact with, the southern portion of the next (as in Nos. 10, 11. and 12.); and it is also remarkable that in almost whatever manner the arrangement was varied, the magnetical order being strictly preserved, the result was the formation of a magnetical *battery*, of *exactly* the same attractive force, which was uniformly equivalent to a deviation of 31° in each of the arrangements, No. 5, 8, 10, 11, and 13. The accession of power gained by all these arrangements above the order of the magnets in No. 3., which is the usual series of compound magnets, points out a method of augmenting the intensity of artificial magnets, by adopting any of these arrangements which promise, from the analogous experiments, to be capable of affording a very great augmentation of intensity. All the preceding experiments, it appears to me, tend to illustrate the probable manner of the action of the magnetic particles in a magnetised body,—each little magnet made use of being supposed to represent a single particle; and they, in a particular manner, point out how the individual magnetisms of each particle, when duly arranged, contribute to the total effect. If this be the case, the action of a magnet in

developing the magnetic properties of any ferruginous body, simply consists in giving arrangement to the magnetic particles. And the manner in which this is accomplished, is, I conceive, very well illustrated, by the experiment I shall next describe.

Let there be several small magnetic needles (those I used were an inch in length), with a small stand of brass for each, holding a fine point, on which the needle may traverse. Arrange them in a row on an east and west line, and at such a distance from each other, that when the needles are in order, and forming a continuous line, their extremities may be about the fiftieth of an inch asunder. In this condition they will be easily arranged (if they do it not spontaneously) in a regular series, with all their north poles one way, when they may be considered as representing the particles of a small magnet, and in a state to afford a striking illustration of the probable manner of action of a strong magnet on a small magnetizable body. For, on passing the north pole of a bar-magnet over the series, beginning at the south end of the row, and carrying the magnet at an inch or two elevation above the needles, according to its strength, in a gradual manner, to a little beyond the other end of the series, all the needles will, in succession, turn round, and finally arrange themselves with their poles in the opposite direction to that they had at the commencement. Now, this effect resembles the change of poles produced in a weak magnet, by passing along it the pole of a strong magnet,—for in both cases the order of the poles is analogous, the pole where the operation terminates being the reverse of the denomination of the pole passed over. And, in like manner, commencing with the needles in disorder, which resembles the condition of an unmagnetized piece of iron, the mere juxta-position of one pole of a magnet, or the passing across it of a single pole, arranges the series in a magnetical order, illustrating the developement of the previously neutralised magnetic principle. But, besides these resemblances, the principles exhibited by these experiments, may be applied to the explanation of the various processes for magnetizing iron, and of several phenomena connected with the mysterious agent in question, which have been generally considered as difficult and obscure.

ART. XXI.—*On the Geographical Distribution of the Vine, (Vitis vinifera.)* By Professor SCHOUW*.

THE circumstances of a local nature affecting a plant which supplies an important article of nourishment to the inhabitants of the earth, and the cultivation of which affords employment to a great number of individuals, cannot fail to be interesting, not merely to the student of botany and geography, but to the economist, the philosopher, and the statesman. As an example of the local relations of a cultivated plant, the vine has here been selected.

Since a detailed account of the distribution of this plant would here exceed our limits, the reader is referred to the works which treat of its cultivation.

The vine does not appear to have a decided preference for any kind of soil in particular. It may be said, however, in general, to succeed better in a dry than in a moist situation. With regard to the management, as is well known, different modes have been adopted; the plant at one time resting on the ground, at another being fastened to stakes, or finding support from other natural objects, such as the mulberry or the elm.

The difficulties which present themselves in ascertaining the

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* The geographical and physical distribution of plants, since the publication of Stromeyer's early essay, has continued to occupy the attention of naturalists, and the works of Brown, Humboldt, De Candolle, Buch, Wahlenberg, &c. are proofs of the interest it has excited amongst the most distinguished botanists of our time. Schouw of Copenhagen, an experienced observer, has, for several years, devoted his attention to this important subject. In 1816, he published a curious essay, entitled, "*Dissertatio de Sedibus Originariis Plantarum*;" and an Essay on the Geography of Plants, by the same author, appeared in Sprengel's "*Neue Endeckungen* for 1821." In 1823, appeared his large work "*On the Geography of Plants*," accompanied with a folio atlas, in which the distribution of plants over the globe is expressed by means of colours. We have read this interesting work with much satisfaction, and have no doubt that our readers will peruse with pleasure such extracts from it as bear on the popular and philosophical parts of this beautiful department of botany. In the present number we have extracted the account of the distribution of the Vine; but the coloured map representing the geographical distribution of this plant, could not be got ready in time. In the following numbers, we shall continue to present our readers with further extracts, and, when necessary, accompany them with notes.—EDIT.

distribution and natural temperature of any plant, are doubled with regard to one which has been cultivated. Whether a cultivated plant occur at any particular place, depends, not merely on climate, but also on the state of the civilisation and industry of a people, their mutual intercourse, and often on their manners, religious tenets, &c. There are always, however, limits fixed by the climate, which neither industry nor art can surmount. To ascertain these is important, both in a theoretical and practical point of view; but even the knowledge of the actual distribution, without regarding its possible enlargement, is by no means devoid of interest. The difficulty, however, is greater, since, in the generality of botanical works, little notice is taken of the cultivated plants, and hence the materials must be collected with great labour from geographical treatises and books of travels. In this inquiry I have confined myself to those portions of the earth's surface where the vine succeeds in the open air, and where its culture forms a branch of industry; for, by extending it to countries where the vine is only reared in gardens or under glass, the result would have been in a great measure arbitrary and indefinite.

Young, in his Travels in France, has determined the northern limit of the vine in that country; it is also indicated in the Flore Française, t. i. in the map of the geography of plants. In Brittany and Normandy the vine is no longer cultivated, and having been succeeded by the apple, cider is there the customary drink. From ancient documents, however, it appears, that in these provinces the vine was formerly reared. The northern limit has, therefore, been perhaps somewhat arbitrarily changed; and it is now, on the western coast, in 47° 20′, about Nantes, or a little to the northward. In the inland parts of the country it ascends to 49° (in the neighbourhood of Paris), in Champaigne 49°–50°, and farther to the east, Young gives the junction of the Mosel with the Rhine (50° 20′) as the boundary; perhaps it reaches even to the 51st degree. In the heart of Germany it is in a somewhat lower latitude; but in Thuringia, Saxony, and Siberia, it is as far north as on the Rhine, or in 51°. The wine, however, in these countries is for the most part inferior. Farther towards the east the limit descends; for although Hungary has much wine, Galicia has none, and hence

it appears to be here in 48°–49° *. In Moldavia the culture of the vine is considerable, as well as in the Crimea, and the southern parts of Russia; but it scarcely advances here beyond the 47th to the 48th degree. In Moscow the vine is only reared under glass. In Western Asia it is cultivated both at the foot of Caucasus, and also at Astracan; the limit may be 47°–48°. In the inner parts of Asia, it is not so easily ascertained. The vine occurs on the other side of the Caspian Sea, in the country of the Turcomans, in Great and Little Bucharia, but is entirely wanting in Siberia. In the eastern part of the Old Continent, the culture of the vine disappears. In Japan, according to Thunberg, grapes do not ripen; neither are they cultivated in China, nor in Cochinchina, although certainly not owing to causes connected with the climate; but perhaps to the very extensive culture of tea, as well as to the restricted intercourse of the Chinese with strangers, and their aversion to adopt foreign customs. Several kinds of Vitis, however, grow wild in Japan, and probably also in China. In the New World, it is singular that the vine is only cultivated in the southern provinces of the United States, although several sorts of Vitis are met with over a very considerable district, which caused the part of America discovered by the ancient Scandinavians before Columbus, to be called the Land of Wine. To the west of the Alleghany Mountains, the culture of the vine seems to extend farther north, being found on the banks of the Ohio in 37°. The north-west coast appears to be more favourable to the vine, for it is reared at St Francisco (38°), and, according to Humboldt, to the north of Monte Ray beyond 37°. The northern limit in Europe and Western Asia is consequently nearly coincident with the parallels of latitude, and does not by any means suffer such deviations as that of the beech-tree. In the New Continent it is a much lower latitude; arbitrary causes may, however, come here into operation, and, in the same way, we must account for the vine not being cultivated in the eastern part of the Old Continent.

The southern limit of the vine in the northern hemisphere, falls, according to Von Buch †, on the island of Ferro (27° 48′), and this must be acceded only, in so far as relates to places at the level

* Wahlenberg, *Flora Carpathorum*, p. lxvii.

† *Allgemeine Ubersicht*, p. 16.

of the sea; for, in Barbary, the vine flourishes only in the northern parts, and a northern exposure is considered the most advantageous *. In Egypt, the culture of the vine is inconsiderable †; and, at Abuscheer in Persia, (29° 2′), the vines, according to Niebuhr, are planted in pits from six to ten feet deep, that the grapes may not be dried up by the heat of the sun. They are found, however, on the island of Bahrein, a little farther south than 27°. In North America, the southern limit appears to be on the west coast, near St Diego, or at the boundary between Old and New California, (32° 39′). In the province of New Biscay the vine is indeed found as far south as 26°, (the Lake Parras, St Luis de la Paz), but probably not at the level of the sea. In New Mexico the limit extends only to Passo del Norte (32° 9′) ‡. In no case does it reach the tropic of Cancer at the level of the sea. In the torrid zone, the vine is found at certain elevations; thus, at St Jago, one of the Canary Islands (15°), and in the Island of St Thomas on the coast of Guinea, being nearly under the equator; this island, however, is mountainous: farther, in the high lands of Abyssinia; and in Hindostan, in the Decan, at a considerable height. In Cumana (10° 27′), the vine, according to Humboldt, bears excellent grapes; yet neither here, nor any where else, in tropical America, not even in the higher lands, can the vine be properly said to be cultivated. In the southern hemisphere, we again find the culture of the vine beyond the tropic of Capricorn, but only where it has been introduced by the colonists; and hence the limits are pretty arbitrary. The vine occurs at the Cape of Good Hope (34°), and some degrees farther north in South Africa; at Conception in Chili (37°); in the interior of South America, in the province of Buenos Ayres, (30°–35°); and in New South Wales (34°); it does not constitute here, however, a separate branch of culture. The limits towards the South Pole cannot be accurately defined, partly because there is little land in the higher latitudes, and partly because there is no cultivation. Since, however, the culture of the vine is not found in the southern part of Chili, and not even

* The vine is extensively cultivated in Morocco, but only on account of the grapes as food, the use of wine being forbidden by the Mahomedan law.

† The cultivation of the vine was more considerable during the time of Cleopatra.

‡ Humboldt, *Essai Politique*, t. ii. p. 441, 442.

in Van Dieman's Land, they certainly do not go beyond the 40th degree of south latitude.

The vine, therefore, occupies two belts on the earth's surface, both in the warmer regions of the temperate zones; its distribution in these, however, is by no means universal, but from deficiency of cultivation in different countries, from the customs, manner of life, and civilization of the people, and other causes, is subject to many interruptions. The elevation above the level of the sea occasions also gaps, and, in this respect, De Candolle * determines the highest limit in 45° of latitude, at 2460 feet; according to Wahlenberg †, it amounts, in the north of Switzerland, to not more than 1700, and in Hungary only to 900 ‡. According to my own observations, 2000 may be assumed for the southern declivity of the Alpine range; for the Apennines and Sicily at the utmost 3000 feet. On Teneriffe, according to Von Buch §, the vine does not ascend above 2500. It is wanting in the high lands of tropical America; and, with respect to the particular spots within the tropics where it occurs insulated, the heights have not been accurately ascertained.

Do we, on the other hand, wish to ascertain those parts of the earth's surface, where the produce of the vine, as well in respect of quantity as quality, attains its maximum, then we have only to look to the south of Europe, and western parts of Asia. Other causes, however, certainly contribute to this besides climate; especially that these regions have always been the principal seat of refinement, and that the vine is probably a native there.

The degrees of latitude affect also the goodness of the grapes and the wine. In northern parts, the wine is more inclined to acidity than in the south, which is sufficiently shewn by a comparison of the Rhenish with the Sicilian or Grecian wines. The strength of the wines also increases as we approach the Tropics. In confirmation of which, Madeira, for instance, is stronger than that of the north. In this respect, however, the latter improves as it gets old. We have farther to remark, that particular localities seem to exercise a decided influence. Thus, the various

* *Mémoires d'Arcueil*, t. v. p. 277.

† *Tentamen de Vegetatione et Climate in Helvetia Septentrionali*, 1813

‡ *Flora Carpathorum*, p. lxvii. et 396.

§ *Allgemeine Ubersicht*, p. 40.

Hungarian wines, (Tokay, for example), are distinguished for strength; the cause of which Wahlenberg seeks to find in the prevailing dry easterly winds. Frequently, in a particular district, or even in a single vineyard, wine of a particular flavour is produced, such as Constantia, Hocheimer *, &c.

It appears from a Table I have published, that the mean yearly temperature of any particular place affords no perfect rule for the distribution of the vine. Thus, the mean temperature of London is higher than that of Zurich, and very little lower than that of Geneva, and yet the former lies nearly 3° beyond the northern limit of the vine, while the two last are within it. Ofen, again, lies $1\frac{1}{2}$° south of it, while the mean temperature is very little more than that of London. In Sicily the vine ceases to grow, on account of the elevation, where the mean temperature is greater than that of London; and this, it appears, is still more strikingly the case in the Canary Islands. The mean temperature of summer has much greater influence. It is higher in Zurich, Geneva, Paris, and Ofen, than in London, or in Sicily, at a height where the mean yearly temperature is greater. It is, indeed, easily comprehensible, that the temperature of that period at which the grapes ripen, must be the most important. Herein lies the cause why the northern limit in Europe does not recede from the pole in our progress eastward, but, on the contrary, is found in a lower latitude in the western than in the middle parts; for, although the mean yearly temperature decreases considerably from west to east, the temperature of summer, on the contrary, encreases even on the same parallel, and much more on the same isothermal line. On that account, the upper limit in Italy, Sicily, and Teneriffe, is proportionally much lower; for, towards the equator, the heat of summer does not increase nearly in an equal degree with the medium temperature. A high summer temperature, combined with a low annual temperature, can, however, only to a certain extent, make a country adapted to the culture of the vine. Thus, the isothermal summer heat of Moscow is 1905, which is higher than in Paris, Zurich, or Geneva, and yet grapes at the former only ripen under glass. It may, indeed, at once be affirmed, that a severe cold, especially a cold

* There are numerous *varieties* of the grape-vine, differing in flavour and other qualities; and the prevailing culture of particular varieties often gives a character to the wine of the district.—Edit.

spring and autumn, must prevent the culture of the vine, however high the summer temperature may be. Frequent night frosts, at the time when the leaves are unfolding, or when the blossom shews itself, and a low temperature in the autumnal months, in which the grapes come to their full maturity, must act injuriously on the vine. Perhaps there is here a concomitant reason why, in North America, the vine does not ascend farther to the north, although the summer temperature is high in proportion to the mean throughout the year.

As the northern limit is chiefly determined by the medium temperature of summer, it is equally so with the southern. In Madeira and the Canary Islands, the mean temperature is not much lower than in Algiers or Cairo, and yet the culture of the vine is very considerable in these islands; whereas in Algiers the temperature is already too high; and in Cairo it is only planted for the sake of its shade. In islands, however, the summer heat is lower than in continents.

It is still more difficult to determine the natural than the artificial distribution of the vine, or, what is the same, the countries where it is a native. Every cultivated plant easily becomes wild, and it is therefore often difficult to decide whether at any given place it may have been native. It is easy to shew, however, that the vine is not originally wild in the New Continent, on the Canary Islands, Madeira, or the Cape. It is farther probable, that this also applies to all the countries of Europe, on this side of the great Alpine range. In Caucasus and the Levant it frequently occurs wild, and every thing concurs to point out this as its original home. In the south of Europe this is more doubtful. It is indeed found in Italy, particularly in the southern parts, in woods, and in such circumstances that there one might be inclined to regard it as originally wild. I cannot, however, affirm this to be altogether ascertained. Historical accounts, and popular traditions, might, perhaps, throw light upon the migrations of this plant. This investigation, however, does not properly belong to our subject.

Within the limits of the vine, wine is the customary drink. In higher latitudes, beer and cider supply its place, and, as a spiritous drink, ardent spirits from grain. In the torrid zone, various plants afford substitutes for wine: thus, in Mexico, the Agave Americana is cultivated, and yields a kind of wine;

Raphia (Sagus) vinifera gives a beverage resembling wine, in Guinea and other places; Rice and the Sugar-cane furnish both Indies with spiritous liquors.

Art. XXII.—*On the Illuminating power of Coal-Gas, and Oil-Gas.* By Andrew Fyfe, M. D. F. R. S. E., Lecturer on Chemistry, Edinburgh.

In my paper, published in the last number of the Journal, I proposed the use of Chlorine, as a means of ascertaining the illuminating power of the Gases derived from the decomposition of Coal and Oil. The few experiments I had at that time performed, left me rather undecided with respect to its accuracy. I have accordingly resumed the subject, and have found, that it promises to be not only one of the most accurate, but the easiest employed indication hitherto used. I have already stated, that oil and coal gas, after being properly purified, are nearly of the same composition, their ingredients being hydrogen, carbonic oxide, carburetted hydrogen, and olefiant gas, the last of which is almost the only source of light, and, in proportion as it varies in quantity, the illuminating power also varies. If this opinion be correct, we have merely to find the proportion of olefiant gas, and we have at once the relative power of illumination.

The method I have described for effecting this is extremely simple. A graduated jar, inverted on a water-trough, must be filled to the mark 50 with chlorine, and fifty measures of the gas under examination then introduced, covering the jar with a paper shade, to prevent the action of light. In the course of about ten minutes the condensation is completed. As chlorine and olefiant gas combine in equal volumes, the diminution, indicated by the ascent of the water, points out the quantity of the latter in 100 of the gas.

With a view of proving the accuracy of this method, after ascertaining the quantity of olefiant gas in different gases, I have tried their illuminating power in the usual way, and the results very nearly coincide.

Exp. 1.—Gas prepared at the Edinburgh Coal Gas Works, from best parrot coal, was found to yield 17 per cent. of olefiant.

Oil-gas, prepared at Mr Milne's, from whale oil, yielded exactly 32 per cent.

Making their illuminating power as 17 to 32, *i.e.* as 1 to 1.8.

The illuminating power of the above gases was tried by burning them with a jet burner, so as to make them give out the same intensity of light; and the quantity consumed was in a certain time as 1.9 coal gas to 1 oil gas, making it 1 to 1.9.

Exp. 2.—Gas prepared at the Coal-Gas Works, contained 14 per cent. olefiant.

Oil-gas prepared by myself contained 26 per cent., making the illuminating power as 14 to 26, *i. e.* as 1 to 1.8. Tried by the consumption it was as 1 to 1.6.

The above experiments were performed on a small scale, in my Laboratory. The following, still more satisfactory, were conducted at the Coal-Gas Works, along with Mr Watson, Manager, and Mr Kirkham, Engineer.

The retorts were charged with 21 cwt. of the best parrot-coal at half an hour after eleven.

At a quarter before twelve the gas yielded 14 per cent. of olefiant, and with a single jet burner and 3 inch flame, consumed 0·656 of foot per hour.

At a quarter before one, it contained 19 olefiant, and the burner consumed 0·52 of foot.

At a quarter before two the olefiant was 17, and the consumpt per hour 0·62 of foot.

At a quarter before three it afforded 14 olefiant, and consumpt was 0·64.

At a quarter before four the gas was coming off very slowly, and contained only 5 olefiant, the burner consuming 0·88 of foot per hour. The illuminating power of this last gas with 5 of olefiant, was tried with another, in which there was 18 per cent. For this purpose they were passed through accurately adjusted meters, and burned with a single jet-burner. Having brought the intensity of shadow from both to be the same, the quantity consumed in a certain time was, as 7 of the former to two of the latter, making the power of giving light as 1 to 3·5. The olefiant gas was as 5 to 18, making it by this test as 1 to 3·6; so that by this experiment, conducted with every degree of care, there is only 0·1 of difference in the results by the two methods.

The results obtained by experiments performed along with Mr Milne, were the same, and in these the illuminating power of the gases was tried under a variety of circumstances. Taking

the average of ten trials, it was fixed at 1 to 1·6. The gases were found, by the chlorine test, to contain coal-gas 15, oil-gas 25 per cent. of olefiant, making their illuminating as 15 to 25, that is, as 1 to 1·6, so that in this also the difference is only 0·1.

The above experiments, I think, are sufficient to warrant the conclusion, that the olefiant gas is the source of light in the gaseous products, derived from the decomposition of coal and oil, so that the chlorine test is an accurate indicator of the illuminating power. Having satisfied myself as to this, I have performed a number of experiments, with a view of fixing that of the gases prepared at the Edinburgh Coal-Gas Works, at the Leith Oil-Gas Works, and by Mr Milne's apparatus.

The Edinburgh coal-gas is made from parrot-coal, in general from a mixture of an inferior with a superior kind; it is, however, occasionally manufactured from that of the best quality. The gas from the former I have, by repeated trials, found to contain 15, that from the latter 17 per cent. of olefiant. I understand it is the intention of the Company to prepare the gas from the best coal, when a sufficient supply can be procured, and with this view they are forming depots of it; but at present they are obliged to use the mixed coal, so that we must consider the coal-gas now employed by the public as containing only 15 of olefiant.

The quantity of olefiant in oil-gas varies considerably, according to the mode of manufacture. When prepared at Mr Milne's, with all possible care, it contained 32 per cent., but some made lately yielded only 25. That prepared at Leith varies from 16 to 17. The illuminating power of the Leith oil-gas, and of the Edinburgh coal-gas, is therefore very nearly the same, while that of Mr Milne varies from 1·6 to 2·1 compared to that of the coal-gas as 1, that is, taking the latter as containing 15 per cent. of olefiant; but if we consider it as having 17 per cent., then Mr Milne's gas will be as 1·4, and 1·8 to the other as 1.

As the composition of the gases varies considerably, it is impossible to fix accurately the illuminating power, but, taking the average of the above trials, it is as 1 to 1·7, or it may be stated to vary from 1½ to 2, compared to that of coal-gas as 1; a conclusion very nearly the same as that at which I arrived in my former paper. (*Suprà*, p. 183.)

In stating the illuminating power, I have chosen Mr Milne's gas as a standard, because, when well prepared by his apparatus, it is, I have been informed, of equal illuminating power with those made from oil, in the different oil-gas establishments in England. From a number of trials which have been made at the different manufactories, he has come to the conclusion, that a burner consuming one foot per hour, gives a light equal to that of eight candles (short sixes) burning with a clear flame, which is the same as that afforded by his gas, when well prepared; that is, containing 32 per cent. of olefiant, which is the highest I have found it to yield.

With a view of confirming the accuracy of my conclusion with respect to the illuminating power of the gases, I have, in company with Mr Milne, compared them with the light afforded by candles; and as the experiments were numerous, and conducted with every possible care, to secure a regularity in the consumption, I am inclined to place the greatest reliance in them, at least as far as we can trust to this mode of determining it. The coal-gas, in all the trials, was that prepared from the mixed coal, and contained 15 of olefiant. The oil-gas was prepared from palm-oil, and contained 25 per cent.

Coal-gas.—Exp. 1.—Argand burner, No. 1., with five holes, and a flame of 3 inches, consumed 2·25 feet per hour, giving a light equal to that of 8·62 candles (short sixes), burning with a clear flame; 1 foot is therefore equal to 3·84 candles.

Exp. 2.—Argand burner, No. 2., with ten holes, consumed per hour, with 3 inch flame, 3 feet, and gave a light equal to that of twenty candles; 1 foot, therefore, equal to 6·6 candles.

Oil-gas.—Exp. 3.—Argand burner, No. 1., with ten holes, consumed per hour, with $1\frac{5}{4}$ inch flame, 1 foot, giving a light equal to that of 6·78 candles.

Exp. 4.—Argand burner, No. 2., with 14 holes, consumed per hour, 1·95 feet with $1\frac{5}{4}$ inch flame, the light being equal to that of 15·34 candles, that is, 1 foot equal to 7·8.

Exp. 5.—Argand burner (of Glasgow), No. 1., with ten holes, consumed 1 foot per hour, flame $1\frac{5}{4}$ inch, the light being equal to that of 7·21 candles.

The conclusion from the above experiments, taking each gas burning under the most favourable circumstances, is as 6.6 to

7.8, that is, 1 to 1.2. And, taking them burning under the least favourable circumstances, it is as 3.84 to 6.78, that is, 1 to 1.7. Taking the coal-gas under the least advantageous, and oil-gas under the most advantageous circumstances, the illuminating is as 3.8 to 7, or as 1 to 2.03.

These experiments were repeated on another night, with the same gases.

Coal-Gas.—Exp. 6.—Argand, No. 1. consumed 1.79 feet per hour, with 3 inch flame, and gave a light equal to that of 7.2 candles, that is, 1 foot to 4.02 candles.

Exp. 7.—Argand, No. 2. consumpt 3 feet, light equal to 13. candles, that is, 1 foot equal to 4.33.

Oil-Gas.—Exp. 8.—Argand, No. 1. consumed, with 1¾ inch flame, 1.46 feet, light equal 6.2 candles, that is, 1 foot equal to 4.24.

Exp. 9.—Argand, No. 2. consumpt per hour 2 feet; light equal to 11.3 candles, or 1 foot equal to 5.64.

Exp. 10.—Argand, No. 1. (of Glasgow) consumed 1.36 feet per hour, flame 2½ inches; light equal to 8.1 candles, or 1 foot equal to 5.95.

The conclusion from these trials, taking each gas under the most favourable circumstances, is as 4.33 to 5.95, that is, 1 to 1.37.

Taking them under the least advantageous, it is as 4.02 to 4.24, or 1 to 1.05.

Taking the coal-gas under the least, and oil-gas under the most favourable circumstances, it is as 4.02 to 5.95, that is, 1 to 1.48.

The above trials give the illuminating power as follows:

Experiments.	Burner.	Coal-Gas.	Oil-Gas.
1. and 3.	1	1	1.7
1. — 5.	1	1	1.8
2. — 4.	2	1	1.18
6. — 8.	1	1	1.05
6. — 10.	1	1	1.48
7. — 9.	2	1	1.3
			6.)8.51
Giving an average of Coal-Gas 1, Oil-Gas			1.42

The gases used in these trials contained the one 15, the other 25 of olefiant, making the illuminating power, by this method, as 1 to 1.66.

I have stated, that the light given by a burner consuming one cubic foot of good oil-gas per hour, has been found by Mr Milne, when it was burnt under favourable circumstances, to be equal to that of eight candles (short sixes). In the experiments just detailed, the average does not amount to above 6, and even in that giving the most light it is only 7.8; so that the gas used must have been of rather inferior quality. Mr Milne accounts for it, by his apparatus being at present not in good repair. Suppose we take gas giving a light equal to that of eight candles, and bring it in opposition to the Edinburgh coal-gas, their illuminating power is, taking the coal-gas at the highest, as 6.6 to 8, that is, as 1 to 1.12; and taking it at the lowest, as 3.84 to 8, or as 1 to 2.09. But taking the coal-gas at the average of all the preceding trials, which is 4.69, then the illuminating power is as 4.69 to 8, or as 1 to 1.7.

From what has now been said, we are still more enabled to judge of the accuracy of the method proposed for ascertaining the illuminating power. By the trials mentioned, it is

By the Consumpt.	By the Chlorine Test.
as 1 to 1.9	as 1 to 1.8
1 to 1.8	1 to 1.6
1 to 1.4	1 to 1.6
1 to 1.35	1 to 1.36

varying in its results only from 0.1 to 0.2 from those given by the other mode.

In having recourse to this test, we are saved the trouble of measuring the consumpt of the gases, and avoid also the uncertainty as to the intensity of the shadow, for unless we are accurate in this, it is impossible to ascertain, with any degree of certainty, the power of giving light. It possesses another advantage, that of finding the illuminating power of gases in different places. If, therefore, it should be adopted, it will be necessary, with a view of avoiding confusion, to fix on some point to commence with, and call the lighting power at this 1. Perhaps it will be best to state it just as the quantity of olefiant gas, that is, if the gas contains 15 per cent., let its illuminating power be termed 15, and so on, according to the proportion.

Thus, I would propose to call the coal-gas of Edinburgh at present offered for sale 15; the oil-gas of Leith 16; that of Mr Milne 35 or 32, according to the proportion of its olefiant gas.

The conclusion at which I have arrived with respect to the illuminating power of coal and oil gas, is the same as that stated in my last paper,—of course, the answer to the question, Can oil-gas compete with coal-gas? must also be the same. The price of the coal-gas of this place is 12s., the lowest at which oil-gas has been sold is 40s., per 1000 cubic feet. If, then, we take the illuminating power as 1 to 2; that is, that one foot of the latter will go as far as two of the former (and this is giving it higher than it is in general made on a large scale in oil-gas establishments), a light which costs the consumer 40s. may be had from coal-gas for 24s. But this, as I have mentioned, is giving it every advantage. If we consider the light given by a burner consuming a cubic foot per hour, as equal to that of 8 candles (short sixes), the illuminating power, compared with Edinburgh coal-gas, is only as 1.7 to 1; consequently a light which costs 40s., can be supplied by the other for a little more than 20s.

Unless, then, oil-gas can be offered for sale at a much lower price than it has hitherto been sold for, it cannot compete with coal-gas.

The preceding remarks must not be considered in a general point of view; they apply alone to the gases as made at Edinburgh, at least with respect to coal-gas. Oil-gas, I have already mentioned, seems, when manufactured on a large scale, to be nearly uniform in its composition *, which is not the case with coal-gas, its illuminating power depending on the coal employed; hence the superiority of the Edinburgh gas.

Dr Henry, in his paper in the Annals of Philosophy for September 1821, has stated the quantity of olefiant gas in a coal-gas; so that we are enabled to fix its illuminating power, and compare it with oil-gas. The gas was prepared from Wigan

* Good oil-gas I have stated to contain about 32 of olefiant gas; whereas that made at Leith has only 16. The inferiority of this gas seems to be occasioned by the imperfect state of the apparatus, several defects having been discovered, but which, I understand, it is intended to remedy immediately, and by which it is expected the gas will be much improved.

Canal coal, at the manufactory of Messrs Phillips and Lee, and collected from an opening in a pipe between the retort and the tar-pit. When taken about half an hour from the commencement of the distillation, the olefiant was in some instances 13, in others 12; but as the distillation proceeded, it was so low as 7; and near the termination it did not contain any: consequently, had this gas been examined from the gasholder, the olefiant must have been considerably below 13 or 12, the good being mixed with the bad. In a former part of the paper, I have given the results of experiments performed at the Edinburgh Coal-Gas Works, with the view of ascertaining the quantity of olefiant in the gas at different times, after the commencement of the distillation.

At ¼th of hour it was	14
At hour and ¼th,	19
At two hours and ¼th,	17
At three hours and ¼th,	14
At four hours and ¼th,	5

The gas, when tried from the gas-holder, yielded 17; for though that given off towards the termination was very bad, yet in the later stages of the distillation it comes off very slowly. The gas, then, of Dr Henry would not perhaps yield above 10, but let us take 12, which I am confident is more than it ought to be, the illuminating power, compared with average oil-gas, would be as 12 to 32, or as 1 to 2.66; if we consider it containing 11, then it is just 1 to 3.

The opinion I have ventured to give, applies to the gases as *now* prepared, for it certainly would be going too far to say that oil-gas *never* can compete with coal-gas. From some experiments I have performed, I have every reason to believe that the illuminating power of oil-gas may be greatly increased; indeed, I have repeatedly succeeded in making a gas having more than three times the power of illumination even of the Edinburgh coal-gas. The experiments on this subject have been performed on a small scale, and are not yet sufficiently numerous to warrant their publication: should circumstances permit me to extend them, they may perhaps form the subject of another communication. I have found, in the making of gas from oil, that that disengaged after the supply of oil is cut off, possesses little

illuminating power. In some cases the olefiant gas did not exceed 3 per cent.; consequently, when added to that which is first discharged, must diminish the proportional quantity in the whole. Could this, therefore, be thrown away, the illuminating power might be augmented. But the same remark applies to coal-gas, that disengaged towards the end of the distillation also yielding very little olefiant gas. It becomes, therefore, an object of importance for coal-gas companies to consider whether they can afford to throw this away, so as to increase the value of the gas exposed for sale. This, I conceive, they can afford to do much more easily than oil-gas companies, because the expence of the raw material is a mere trifle compared to that of oil; and though, by diminishing the length of time of each charge, the retorts require to be more frequently filled; yet for the time lost in doing this, there might be a recompense in the saving of the retorts themselves, and the time spent in making a small quantity of bad gas being occupied in getting one of superior quality.

The experiments on the illuminating power of the gases performed with Mr Milne, lead to a very important question, Which is the best method of consuming them, so that the greatest light may be got from a certain quantity?

No. 1. Argand burner, with five holes, consumed 2.25 feet of coal-gas per hour, giving a light equal to that of 3.84 candles for each foot consumed. No. 2, with double the number of holes, consumed 3 feet, the light being equal to that of 6.6 candles per foot.

In another trial, the Argand No. 1. consumed 1.79 feet, giving for each foot a light of 4.02, whereas the consumpt of No. 2. was 3 feet, and the light that of 4.33 candles per foot.

Similar results were obtained with oil-gas.

No. 1. Argand burner, with ten holes, consumed 1 foot, giving a light equal to that of 6.78 candles. No. 1. (of Glasgow), with the same number of holes, but having a larger cylinder, consumed the same quantity, but gave a light of 7.21 candles; whereas the burner No. 2. with fourteen holes, consumed 1.95 feet, the light for each foot being that of 7.8 candles.

In another trial, No. 1. consumed 1.46 feet, and gave a light

equal to that of 4·24 candles per foot. No. 1. (Glasgow) consumed 1.36 foot, the light being equal to 5.95 candles; and No. 2. consumed 2 feet, the light being that of 5.64 candles.

It appears, then, that the light afforded by the consumpt of equal quantities of gas, depends much on the size of the burner. The burners No. 2. seem to be better than No. 1.; but it would require an extensive series of experiments on this subject before any satisfactory conclusions can be drawn; and I conceive it is a subject well worthy the serious consideration of oil-gas and coal-gas establishments.

Note.—Since most of the preceding experiments were performed, a letter has appeared from Professor Leslie to the Coal-Gas Company, in which the illuminating power of the gases is stated at 1 to 1.5, rather less than the average conclusion at which I have arrived. Of the accuracy of the mode of judging of the intensity of the light I cannot speak, not being acquainted with the method adopted for preventing any irregular action of heat on the photometer; but, allowing it to be a correct one, let us try if we can account for the discordance in the results.

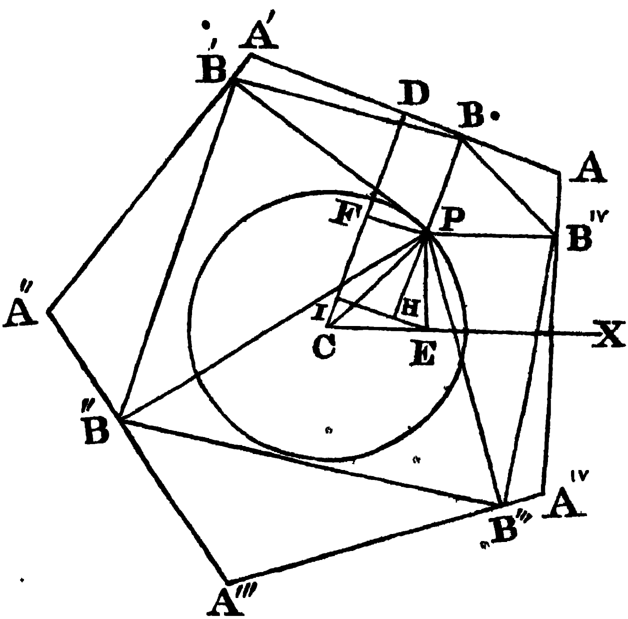
In drawing my conclusion, I have fixed on the gas containing 32 per cent. of olefiant gas, compared to the coal-gas as having 15 and 17; and hence I make the illuminating power as 1 to 2, and as 1 to 1.8. It has been already stated, that the gas lately made by Mr Milne is of inferior quality, owing to his apparatus being out of repair, in which state, I am informed, it has been for some time past. Suppose, then, that the gas given to Professor Leslie was similar to that on which some of my experiments were performed, and which contained 25 olefiant gas, while the coal-gas contained 15, the illuminating power should be as 1 to 1.6; if the latter had 17, it would be as 1 to 1.47; both of which results very nearly agree with those of the Professor.

ART. XXIII.—*A general Theorem relating to Regular Polygons.* By WILLIAM WALLACE, F.R.S.E. Professor of Mathematics in the University of Edinburgh.

THEOREM.

LET there be any regular polygon of n sides, described about a circle whose radius is r, and let α denote the angle at the centre, subtended by a side of the polygon; let the distance of any point within the polygon from the centre be v, and from that point let perpendiculars be drawn to the sides of the polygon: The area of the rectilineal figure formed by straight lines which join the bottoms of the adjacent perpendiculars, is equal to

$$n\{\tfrac{1}{2}\, r^2 \sin \alpha + \tfrac{1}{4}\, v^2 \sin 2\alpha\}.$$



Taking a particular case, let A A′ A″ A‴ A$^{\text{IV}}$ be a regular polygon of five sides: From P, any point within the polygon, let perpendiculars PB, PB′, PB″, PB‴, PB$^{\text{IV}}$ be drawn to the sides, and let B B′, B′B″, &c. be joined, so as to form the figure B B′B″B‴B$^{\text{IV}}$, let α = angle of 72°, r = radius of inscribed

circle, and $v = PC$, the distance of P from C the centre: The area of the figure $B B' B'' B''' B^{IV}$ is equal to

$$5\{\tfrac{1}{2} r^2 \sin \alpha + \tfrac{1}{4} v^2 \sin 2\alpha\}$$

From the centre C, draw a straight line CX perpendicular to AA^{IV}, a side of the polygon, and taking this line as an axis, and C as the origin of the co-ordinates, draw PE perpendicular to CD, and put $CE = x$, $PE = y$: Draw the radius CD perpendicular to AA'; draw PF and EI perpendicular to CD, and PH perpendicular to EI.

It is manifest, that

$$PB = FD = CD - CF = CD - CI - PH.$$

Now, $CI = CE \cos XCD = x \cos \alpha$, and $PH = PE \sin PEH = PE \sin XCD = y \sin \alpha$; therefore,

$$PB = r - x \cos \alpha - y \sin \alpha.$$

If we now consider that α is the angle which a radius parallel to the perpendicular PB makes with the axis CX, it is easy to see, that whatever be the number of sides of the figure, if the perpendiculars PB, PB$'$, PB$''$, &c. be denoted by p, p', p'', &c. we shall in every case have

$$\begin{aligned}
p &= r - x \cos \alpha - y \sin \alpha, \\
p' &= r - x \cos 2\alpha - y \sin 2\alpha, \\
p'' &= r - x \cos 3\alpha - y \sin 3\alpha, \\
p''' &= r - x \cos 4\alpha - y \sin 4\alpha, \\
p^{IV} &= r - x \cos 5\alpha - y \sin 5\alpha, \\
&\&c.
\end{aligned}$$

the number of equations of this form being equal to the number of sides of the figure.

Taking now the products of the adjacent perpendiculars, and their equals, and putting c, c', c'', &c. for $\cos \alpha$, $\cos 2\alpha$, $\cos 3\alpha$, &c.; also s, s', s'', &c. for $\sin \alpha$, $\sin 2\alpha$, $\sin 3\alpha$, &c. and observing that

$$\begin{aligned}
\cos n\alpha \cos (n+1)\alpha &= \tfrac{1}{2}\{\cos \alpha + \cos (2n+1)\alpha\} \\
\sin n\alpha \sin (n+1)\alpha &= \tfrac{1}{2}\{\cos \alpha - \cos (2n+1)\alpha\} \\
\sin n\alpha \cos (n+1)\alpha + \cos n\alpha \sin (n+1)\alpha &= \sin (2n+1)\alpha
\end{aligned}$$

we get, when the figure has five sides,

$$\begin{aligned}
p\,p' &= r^2 - (c + c')\,rx - (s + s')\,ry \\
&\quad + s''\,xy + \tfrac{1}{2}c''\,(x^2 - y^2) + \tfrac{1}{2}c\,(x^2 + y^2), \\
p'\,p'' &= r^2 - (c' + c'')\,rx - (s' + s'')\,ry \\
&\quad + s^{\text{IV}}\,xy + \tfrac{1}{2}c^{\text{IV}}\,(x^2 - y^2) + \tfrac{1}{2}c\,(x^2 + y^2), \\
p''\,p''' &= r^2 - (c'' + c''')\,rx - (s'' + s''')\,ry \\
&\quad + s'\,xy + \tfrac{1}{2}c'\,(x^2 - y^2) + \tfrac{1}{2}c\,(x^2 + y^2), \\
p'''\,p^{\text{IV}} &= r^2 - (c''' + c^{\text{IV}})\,rx - (s''' + s^{\text{IV}})\,ry \\
&\quad + s'''\,xy + \tfrac{1}{2}c'''\,(x^2 - y^2) + \tfrac{1}{2}c\,(x^2 + y^2), \\
p^{\text{IV}}\,p &= r^2 - (c^{\text{IV}} + c)\,rx - (s^{\text{IV}} + s)\,ry \\
&\quad + s\,xy + \tfrac{1}{2}c\,(x^2 - y^2) + \tfrac{1}{2}c\,(x^2 + y^2).
\end{aligned}$$

Hence, by adding, we obtain

$$\begin{aligned}
&pp' + p'p'' + p''p''' + p'''p^{\text{IV}} + p^{\text{IV}}p = \\
&\quad 5r^2 + \tfrac{5}{2}c\,(x^2 + y^2) \\
&\quad + (c + c' + c'' + c''' + c^{\text{IV}})\,\{\tfrac{1}{2}(x^2 - y^2) - 2rx\} \\
&\quad + (s + s' + s'' + s''' + s^{\text{IV}})\,\{xy - 2ry\}.
\end{aligned}$$

Because the circumference is divided into five equal parts at the extremities of the arcs α, 2α, 3α, 4α, 5α, by known properties of the circle, the sum of their cosines c, c', c'', c''', c^{IV}, is $= 0$, and the sum of their sines s, s', s'', s''', s^{IV}, is also $= 0$; hence the terms in the preceding expression which are multiplied by these sums, must vanish: thus we have simply

$$p\,p' + p'\,p'' + p''\,p''' + p'''p^{\text{IV}} + p^{\text{IV}}\,p = 5\{r^2 + \tfrac{1}{2}\,(x^2 + y^2)\cos\alpha\}.$$

Draw PC to the centre, and put $\text{PC} = \sqrt{(x^2 + y^2)} = v$, and, instead of supposing the figure to have five sides, let us suppose it to have n sides; then, by the very same mode of reasoning, we shall find that

$$p\,p' + p'\,p'' + p''\,p''' \ldots + p^{(n-1)}\,p = n\,(r^2 + \tfrac{1}{2}\,v^2\cos\alpha);$$

and multiplying both sides by $\frac{1}{2}\sin\alpha$, we have

$$\tfrac{1}{2}\sin\alpha\,(pp' + p'p'' + p''p''' \ldots + p^{n-1}\,p = \tfrac{n}{2}\,(r^2\sin\alpha + \tfrac{1}{2}\,v^2\sin\alpha\cos\alpha)$$

Now, the lines PB, PB′, PB″, &c. being perpendicular to the sides of a regular polygon, the angles they make about the point P will manifestly be equal; and because their number is n, each will be $= \frac{360^\circ}{n} = \alpha$, therefore the triangle $\text{PB}\,\text{B}' = \frac{1}{2}\,p\,p'\sin\alpha$, the triangle $\text{PB}'\,\text{B}'' = \frac{1}{2}\,p'\,p''\sin\alpha$, and so on: Therefore, the

sum of the triangles, that is the figure B B′ B″ B‴ B$^{\text{IV}}$, is equal to the sum of the products, that is, to

$$\tfrac{1}{2}\,(p p' + p' p'' + p'' p''' + p''' p^{\text{IV}} + p^{\text{IV}} p)\sin\alpha$$

which has been proved equal to

$$\frac{n}{2}\,(r^2 \sin\alpha + \tfrac{1}{2}\, v^2 \sin\alpha \cos\alpha)$$

that is, to $$n\,(\tfrac{1}{2}\, r^2 \sin\alpha + \tfrac{1}{4}\, v^2 \sin 2\alpha),$$
which is the theorem in question.

Corollary.—If two circles have the same centre, and a regular polygon be described about the outer circle, and from any point in the circumference of the inner circle perpendiculars be drawn to the sides of the polygon, the figure formed by joining the bottoms of the perpendiculars will have an invariable magnitude.

For then the expression for the area will be composed of constant quantities.

Art. XXIV.—*Letters from* Dr Oudney; *from* Mr Bowdich; *and from* Captain Parry.

I.

[In the London Quarterly Review, many interesting details have been published regarding the African Journey of our friend and pupil Dr Oudney. The Letters we now lay before our readers form part of a series, which we intend to continue as opportunity offers.]

Dr Oudney to Professor Jameson.

My dear Sir, *Murzouk, June* 6. 1822.

I intended writing you long ago, but my time has been so occupied with such a variety of objects, that really I could not. I am much disappointed in the climate of Fezzan, and agreeably so. The temperature under cover is not much greater than in other places on the same parallel of latitude. Now it is June, and the maximum daily heat is seldom above 90°. The minimum is about 80° or 78° Fahr. The air is dry, as, indeed, several of my instruments shew too well, for the ivory on several

has shrunk so that they are rendered useless. The tube of one attached thermometer is bent like a bow, from being confined; in another, the pressure of the glass has broken the brass-clasps. The hygrometer of Kater generally stands at 38, or 38.5. But instruments affected by the hygrometrical state of the air, and by sand, are useless in climates such as this. I mention sand, for instruments that act by delicate wheels are very soon deranged, and disappoint the scientific inquirer. The barometer varies a little; there is a change particularly from 11 A. M. to 8 or 9 P. M.; the mercury becomes depressed in general about the 20th part of an inch, sometimes a 10th. During northerly winds the mercury rises, and, in general, the stronger the breeze the greater the rise. From that I have been able to predict strong breezes from that direction. On the contrary, southerly winds cause depression, and that commonly in proportion to the violence of the wind. I have not yet calculated the mean height, but it must be about 28.500 inches at the temperature of 80° Fahr. Water boils at 207 Fahr. both which give a considerable elevation. I have anxiously searched for springs, but have found none fit for an accurate result. The whole of the country is a spring, if I may use the expression, for water comes bubbling up on digging a few feet. Its temperature, then, is affected by the earth, and the heat indicated of no use in determining the mean temperature of the place, and consequently its elevation. The abundance of water in a country where it never rains, and where no dew falls, is curious and interesting to the philosophic inquirer. It is not generated in the earth, and it cannot be supplied by the sea. From whence then does it come? Is it from the tropical rains? Or rivers lost in the earth? The supply is constant, and the wells yield as much at one time as at another. The supposition that appears most probable to me is, that the countries to the southward are much higher than this; that the rain during the rainy season penetrates a considerable way into the soil, till it meets strata resisting its descent; and that it then flows along them like a river, to countries far distant. My explanation may be confused, but, on reflection, I think it the most probable. There is another interesting feature in Fezzan,—the continual formation of salt on the surface of the sand. In travelling along, the different sta-

ges of the process are very distinct; first a thin crust, like hoar frost, that gives the crackling sound of soft ground lately frozen over; it increases gradually in thickness, till it arrives at that of many inches, sometimes even a foot.

At present I will not enter into any explanation, but shortly I will be able, I hope, to enter on this subject, and others on the geological structure of Fezzan. The courier is now waiting, and camels, that are to convey me to Ghraat. My worthy friend Clapperton and I are just setting out there, that we may render our delay here as serviceable as possible. The Marick country is perhaps interesting only on account of its inhabitants, who appear a brave and independent race. He and I have been at Zuela, and both could not but be struck with the general uniformity of scenery and structure. He desires to be kindly remembered to you, and will write you very soon. We expect to be able to set out for Bornou in two or three months. Excuse this hasty letter; and believe me, &c.

Dr Oudney to Professor Jameson.

My Dear Sir, *Murzouk*, 19*th Sept.* 1822.

When I wrote you last, we were just departing for the Zwenich country; I hope in about a fortnight after this we will be on our way to Bornou. We found our journey to Ghraat very pleasant, and our reception was very flattering. We were among a brave warlike race, exceedingly superstitious, yet sensible; uncommonly strict Mahommedans, yet liberal in their ideas. The geology and botany of the country nearly the same as Fezzan, which I am not able to enter into at present. We made long excursions in Waday Ghrurbi and Wadies Shiati, which Captain Lyons incorrectly joins; in our cruize we examined the Trona Lake. It is situated in the midst of amazingly high sand-hills, that run for several hundred miles to the westward, and lie between Wadies Shiati and Ghrurbi. It is in a small valley which runs nearly ESE. and WNW. The north and south sides are bounded by hills of sand about 400 feet high. The bottom is a fine sand, on which are found the Agoul (apparently a species of Ulex), and a downy grass.

Near where we entered the valley, there is a cluster of date-palms, and a small lake, from which impure trona is obtained. On the western side is the trona lake, surrounded by date-trees, and its banks and marshy borders covered, on almost all sides, by the grass I have mentioned, and a tall juncus. It is about half a mile long, and nearly 200 yards wide, of very inconsiderable depth at present (July), from the evaporation of the water, and many places are dry now which are covered in the winter and spring. The trona is deposited in cakes at the bottom of the lake, when the saturation is at a certain extent. The cakes are of various degrees of thickness, from the finest film to several inches. The thickest I could find was not more than ⅘ths of an inch, but at the beginning of winter, when the water begins to increase, it is of the thickness I have mentioned, and it is then said to be ripe. The surface next the earth is not unequal from crystallisation, but rough to the feel, with numerous rounded asperities. That next the water is generally found studded with small beautiful cubical crystals of muriate of soda; the line of junction is always distinct, and the one is easily removed from the other. The upper surface, when not covered with the muriate, is composed of a congeries of small tabular pieces, joined in every position; when the mass is broken, there is a fine display of acicular crystals, often radiated. The surface of the water is covered in many places with large thin sheets of a carbo-muriate of soda, giving the whole the appearance of a lake partially frozen over: film after film forms, till the whole gets of considerable thickness. The soil of the lake is a dark brown sand, approaching to black, of a viscid consistence, and slimy smell, and on the lately uncovered surface, near the banks, a black substance, like mineral tar, is seen oozing out.

The water begins to increase in the winter, and in the spring it is at the maximum. The trona is best about the commencement of winter, but disappears entirely in the spring.

The lake has diminished considerably in size within the last few years, and if care be not taken, the diminution will soon be much greater: plants are making rapid encroachments, and very shallow banks are observable in many places. On inquiry, we found the quantity of trona had not sensibly diminished for the last ten years; perhaps it may appear so, from there always

being sufficient to answer every demand. The quantity annually exported amounts to between 400 and 500 camel-loads, each equal to about 4 cwt.,—a large quantity, when the size of the lake is considered. It is removed only when a demand comes; then a man wades in, breaks it off in large sheets, which he easily does; hands it to others outside, who are ready to remove all foreign matters, and pack it in the setose bases of the palm-leaves. The water in the valley is good, and if a well be dug on the very border of the lake, the water is also good, and sensibly free from saline impregnation. There are a great many springs in the Wadey Shiati, and a number in the Wadies about Ghraat; all were so exposed, that it was not possible to get any correct observations. The wells also are so discrepant that no information could be obtained. The maximum heat of Murzouk 105° Fahr., a great difference from Captain Lyons. The greatest diurnal change 15°, and that very uniform. Hygrometer shorter, from 360° to 410°,—an amazing small range. My worthy friend Lieutenant Clapperton, sends his kind regards, and promises to write you from Bornou. We are in the best of health, and stood our summer's travelling exceedingly well. I remain yours very sincerely, &c.

II.

Extract of Letter from Mr Bowdich to Professor Jameson.

[The following is the last letter we received from the late enterprising but unfortunate traveller Bowdich. His melancholy fate, deeply regretted by the learned throughout Europe, affords another proof of the wretched climate of that part of Africa where he lost his life, and of the extreme danger of encountering it, even to those who, like Mr Bowdich, were, by previous knowledge and experience, well prepared to provide against its deleterious effects. His observations, drawings, and collections, we believe have reached Europe, having been brought to England by the accomplished and interesting partner of all his joys and of all his sufferings—Mrs Bowdich.]

Having unexpectedly procured a letter to the Vicar of Caniçal, about 15 miles from Funchal, and the last village towards the eastern end of the island, from which it is not much more than three miles distant, I hastened to explore its environs. I had

followed a rough track, on the margin of shallow cliffs of alternations of tufa and basalt, for about a mile and a half, when we reached a depression, more like a basin than a plain, covered with a deep bed of loose and agglutinated sand. These sands have in some degree been fixed or bound by the numerous branches of forest-trees they have enveloped; for these branches, (which have preserved their lateral twigs), are so numerous, that they are spread over the surface, as well as beneath it, like a net-work of stoloniferous roots. It is scarcely possible to set the foot to the ground without treading on them. Both the branches and the trunks, (which stand on their roots in their natural position), are incased in a thick hard sheath of agglutinated sand, which has followed the external configuration of the wood like a cast. In some instances the wood has entirely perished, and the envelopes are found void, like tubes; but most frequently the wood is found within, as a distinct mass, and has become sufficiently siliceous to scratch arragonite (Figs. 108, 109 *). The tallest fragments of trunks reach about a foot above the surface of the sand; how far beneath it I cannot say. There were two or three as thick as my body. Sometimes imbedded in the envelopes of the wood, but generally in the loose sand of the surface, were innumerable fossil shells, intermingled promiscuously; two species terrestrial; the third belonging to a marine genus.

The Delphinula, Fig. 110. *a*, *b*, is the Delphinula sulcata of Lamarck, only known in the fossil state, and found at Grignon. But the Helices belong to the group Lamellatæ of De Ferussac's sub-genus Helicostyla. The smaller species Fig. 111. is globose, but the larger, Fig. 112. *a*, *b*, *c*, (which is ¼th inch in its greatest diameter, and $\frac{7}{10}$th deep), has the last whorl compressed or flattened. There are several helices still smaller than the former, with the umbilicus exposed; but this is merely because the plate which covers the columella is not entirely developed, and I have not the least doubt of their being young shells of the first-mentioned species. These shells are perfectly distinct from the existing helices of Madeira, (Figs. 38, 39, 40, 43, 44.), of which there is not one to be found in the neighbourhood.

* These, and the other Figures noticed in this page, refer to drawings in the author's possession.—Edit.

Al. the branches and trunks appear to belong to the same sort of tree, (of which there seems to have been a small forest in that spot), and that evidently a Dicotyledon; but more than this, I do not think our present knowledge of the comparative anatomy of timbers is sufficiently advanced to determine *. The sand is calcareous, whether from the destruction of fragments of transition limestone (found beneath the basalt at St Vicente) in the bed of the ocean, or from comminuted shells, I will not venture to pronounce, although I incline to the former opinion. The carbonate of lime in the sheaths or envelopes of the wood, bears a greater proportion to the silex than in our common mortar, than which their substance is much harder, for it amounted to 43 per cent. There is much ferruginous sand with that thus thrown up, resulting from the destruction of the masses of red tufa constantly falling from the cliffs into the sea. On the western limit of this local deposit are large globules of basalt, (which, from their concentric form, and other appearances, have evidently been in a fluid state), lying loose upon the soil, from the tufa (in which they are still found imbedded at greater height), having been washed away from them. On such a soil the vegetation must be wretched; a mesembryanthemum, and an orobus were the only plants that existed, or rather languished there.

Having described this locality to the best of my ability, I leave abler geologists to draw the conclusions. But, perhaps, I may be allowed to submit, *First*, That it has evidently been an irruption of the sea, from the heaps of *terrestrial* shells mingled with the *marine*, and from the trees being found standing on their roots, and not deposited promiscuously in detached fragments, or flattened, as they would be, had they been transported thither, or had they been subjected to any pressure from a superincumbent stratum afterwards removed. *Secondly*, It is clear that this must have happened after the Atlantic had lost that considerably higher level which the oysters and marine shells, found 300 miles inland, in the Blue Mountains of America, would seemingly indicate; for the deposit (extending about ⅜ths of a mile in each direction) is bounded by hills and small peaks rising several hundred feet above it (composed of the same

* Specimens of these lignites will be sent to the Geological Society.

tufa in which the sand and hills are deposited, and in the soil of which this small forest must have been growing), which peaks and elevations present no traces of sand on their surface, or elsewhere above the higher level of that in the flat, *i. e.* about 250 feet, or thereabouts, Pl. X. Fig. 2. Seeking for that explanation which rests on the fewest and simplest causes, it occurred to me, when I first reached this bed of sand (which was on the southern side, where it is level with the water's edge), that there might have been no irruption or deviation of the level of the sea, but a subsidence of the tufa strata, (like that of the shores of Alexandria, which, according to Dolomieu, are a foot lower than they were in the time of the Ptolemies), the natural consequence of gravity, or from one of those slips so frequently evident along its coast, which led to a deposit of calcareous sand on the borders of the sea; which sand, from its extremely fine grain, was readily dispersed by the winds, until it reached the north side of the island, (for it is barely $\frac{3}{4}$ths of a mile broad in this part), where the drift-line of the sand, with the tufa on which it rests, is about 50 feet above the sea. But, then, should we find the *marine* shells in such heaps at the height of 250 feet? Would the sand have been so firmly agglutinated as it is in the indurated sheaths which envelope the trunks and branches of the trees? and could there be one regular or dip-line descending S. 30 E.? I cannot help thinking now, that there must have been an irruption of the sea from the northward, covering both this small flat, and that already described in Porto Santo, (where a marine shell, an Ampullina, is also intermixed with the Helices), and depositing the bed of sand on both. However, I have performed the most important part of my duty, by particularising the fact as well as I am able, and will therefore say no more. The high tufa cliffs on the north side of this part of the island, behind Faniçal, are broken off abruptly in their whole depth towards the sea, and presented numerous dip-lines of strata, deeply inclined to the south from these broken faces, Plate X. Fig. 1. as if a considerable part of the island had been broken off, or worn away on that side, which would seem to have been formed by a crater now lost in the ocean, to the northward.

EDWARD BOWDICH.

III.

Mr Merricks to Professor Jameson.

[In a former Number of the Journal, we gave an account of Mr Merricks' plan for blowing up ice, which promised to be useful in many situations, particularly in the Arctic and Antarctic Seas. From the following letter sent to us by Mr Merricks, it would appear that Captain Parry, if occasion offers, will carry it into effect in the Arctic Sea.]

Roslin Gunpowder Mills,
10th August 1824.

Sir,

The letter I received this morning, through your goodness, was from Captain Parry, and dated from "Davis' Straits, 1st July."

I had written him before he left England, with an account of some experiments I had made in blowing up ice with gunpowder, giving him a description of my apparatus, &c., and mentioned at the same time to him, that I thought it might be of use in the event of his having to cut his way at any time through the ice, and might probably supersede the use of saws and axes. He thanks me for my commnnication, and says, "I now beg leave to offer you my very best acknowledgments for your kindness, and to assure you, that I shall be very glad to avail myself of your suggestion," &c. &c. I sincerely wish he may find it of service, should he stand in need of it; at any rate he means to give it a fair trial. I remain, sir, yours respectfully,

John Merricks.

IV.

Extract of a Letter from Capt. Parry to Professor Jameson.

Hecla, Davis' Straits, Lat. 69,
July 1. 1824.

My Dear Sir,

As you were kind enough to give me permission to trouble you with a few lines by the transport, I gladly avail myself of that opportunity, which is shortly about to occur, to acquaint you with our welfare thus far on our voyage, if indeed our voyage may be said to have commenced. The advantages we derive from the plan of having a transport with us are very great, for our own ships could not, with safety, cross the At-

lantic so loaded as our necessities require, in order to enable us to proceed with perfect confidence in our resources. These we can, now, without much pinching or inconvenience, extend to a period of three years and a half from this time. I trust that, with the continued blessing of God upon our endeavours, we may do something worthy of so liberal and splendid an equipment.

I am very much pleased with your friend and pupil Dr Neill, with whom I am making every arrangement in my power to promote the interests of natural history in each of its departments; for, though I can lay no claim to acquirements in this way myself, I am most anxious to leave nothing undone which may, in any way, be beneficial or interesting. We are making every preparation for the collections of specimens, which may at once save time and trouble in making them, and preserve them in the best order for examination and description hereafter. I am, &c. F. PARRY.

ART. XXV.—*On the Detection of Minute Quantities of Arsenic in mixed Fluids* *. By ROBERT CHRISTISON, M. D. F. R. S. Professor of Medical Jurisprudence in the University of Edinburgh.

THE object of this paper is to estimate the value of the liquid tests for arsenic, when dissolved in mixed vegetable and animal fluids, and of the various processes which have been proposed for correcting the changes thus produced in the action of the tests; and, finally, to determine, what mode of analysis is at the same time the simplest, and may be applied under all circumstances. The author has extended the researches of Orfila, and those on the modifications caused in the action of the tests, by the co-existence of animal and vegetable fluids; and shews, by the instances of broth, tea, coffee, porter, Port-wine and milk, that, when the fluid is complicated, or deeply coloured, and the arsenical solution of such moderate strength as may be usually looked for in medico-legal investigations, the four best liquid tests, namely, lime-water, the ammoniacal sulphate of copper,

* The above is a condensed view of Dr Christison's interesting Memoir in the Edinburgh Medical Journal for July 1824.

the amoniacal nitrate of silver, and sulphuretted hydrogen, are almost or absolutely useless. He next proves, that no advantage is to be derived from the plans which have been proposed for restoring the true action of the tests, by destroying the colour of the fluids. These are two in number; the one advanced by Orfila in 1821; the other by Mr Phillips in January last. The former chemist proposes to destroy the colours by chlorine; the latter by digestion with animal charcoal. But Dr Christison finds, that, after the action of chlorine, the colour is seldom altogether, or sufficiently destroyed; that the process does not take from the fluid the power, it often possesses, of retaining the arsenical precipitate in solution; and that, in some decolorised fluids, containing no arsenic, some of the tests cause precipitates very similar to those produced in pure arsenical solutions. He likewise finds, that the process by digestion in charcoal is sufficient, because, if the solution is not very strong, the charcoal removes all, or nearly all, the arsenic, as well as the colouring matter; and, if the solution be strong, it does not always lose its property of retaining the arsenical precipitates dissolved. He then proceeds to examine the processes which have been recommended by Rose and Rapp for discovering arsenic, when intimately mingled with the animal textures, and which might also be applied to the residue by evaporation of mixed fluids, in which the common tests do not act characteristically. These processes it is unnecessary to mention. They are founded on the possibility of arsenic so combining with animal matters, as to resist the solvent power of boiling water. But Dr Christison has found, that, after careful digestion in water, no arsenic can be detected by either process; and, farther, that the process of Rapp is otherwise insufficient, when the quantity of arsenic is small, in which circumstance alone, the ordinary means of analysis are inadequate.

In the last place, he describes the method which he has found easiest of execution, and most generally applicable, for detecting arsenic dissolved in mixed fluids, or mingled with such solids as are incapable of forming with it an insoluble compound. Minute directions are given for the sake of the inexperienced; but we shall notice only the essential parts, and the result of his experience with respect to the delicacy of the method. Having observed, that the sulpheretted hydrogen, though it seldom acts

characteristically on diluted solutions of arsenic in mixed fluids, nevertheless always throws it down of some colour or other, even when the proportion of the poison does not exceed an 8000th part, he proposes to employ this test, with the view of procuring the arsenic in a convenient form for being subjected to the decisive process of reduction. "The suspected matter, if solid, is to be divided into minute fragments, and boiled briskly in two or three successive portions of pure water. The fluid, whether originally such, or procured by digestion from the solid matter, is then to be subjected in a deep, narrow glass, for half an hour, to a brisk stream of sulphuretted hydrogen gas. In many cases, however, it will be necessary to premise the two following preparatory steps, before transmitting the gas; and, as we can seldom know beforehand whether these steps are requisite or not, it may be right to resort to them in every case. The first precaution is to add a little acetic acid to the fluid. By so doing, the influence of any free alkali that may exist in it is counteracted; and several organic principles, which might impede the subsequent separation of the precipitate, are coagulated. The second precaution is to boil the fluid for a few minutes; by which means some matters are separated that the acetic acid could not throw down entirely; and any carbonic acid existing in it is driven off. The presence of carbonic acid in considerable quantity, by impeding the solution of the sulphuretted hydrogen, prevents its action on the arsenic, if the proportion of arsenic be small. The fluid is then to be filtered."—"When the stream has been continued a sufficient length of time, there is either a precipitate formed, or the fluid acquires a yellowish milkiness, which passes to a distinct precipitate as soon as the excess of sulphuretted hydrogen is driven off by heat. It is always right to boil, before attempting to separate the matter thrown down; as the precipitate then becomes much more distinct, and falls to the bottom more readily. When the filtration is finished, and the filter has been gently compressed between several folds of bibulous paper, the precipitate is to be scraped off with a knife, and dried on a bit of smooth paper, at a temperature somewhat above 212°." The most advisable mode of subjecting this to the test of reduction is the following: The best flux is the black flux, and the best instru-

ment a glass tube, closed at one end, open at the other, about three inches long, and varying from a fourth to an eighth of an inch diameter, according to the bulk of the material, which should not fill above two-fourths of an inch of the tube. The best mode of applying heat is by the alkohol lamp, as recommended by Mr Phillips. "The true arsenical crust is known by the following physical character. Its outer surface next the tube exactly resembles highly polished steel. Its inner surface, (which is best seen by scratching the tube with a file at the lower margin of the crust, and snapping it across), is precisely like the fracture of fine steel, if the quantity is considerable; if it is minute, it has a dull bluish-grey appearance, but, before a microscope of four or five powers, appears brilliant and crystalline, like the fracture of steel. Occasionally, when very minute in quantity, it appears botryoidal, and not brilliant, even before the microscope: in that case, the part of the tube to which it is attached, should be coarsely powdered, and heated anew in a tube of less diameter. It is scarcely possible for any one to mistake these characters, particularly if he has ever seen an arsenical crust before; but, to prevent all possibility of error, the analysis may be concluded with the following experiment: The part of the tube to which the crust is attached being broken into fragments, is to be left for some hours in a watch-glass, containing a dilute solution of the ammoniacal sulphate of copper, and covered to prevent evaporation. In four or five hours, the metallic crust will become grass-green; or, if very minute, it will be discoloured, and a brilliant grass-green crust formed on the surface of the liquor. The simple evaporation of the fluid will cause the formation of a crust on its surface, though no arsenic be immersed in it. But in that case its colour is pale-blue *." The author concludes by stating, that the evidence thus procured is quite unimpeachable; that the process is the most convenient yet proposed; that it is probably applicable to all cases without exception, as he has found it to answer with the most complicated fluids he could select, namely, broth, tea, with cream and sugar, coffee similarly made, porter, Port-wine and

* If portions of the crust are simply exposed to the air, they soon acquire a greyish-black colour on the surface; a character also indicative of metallic arsenic. —Edit.

milk; and that it is sufficiently delicate for all medico-legal purposes, as it will detect satisfactorily a quarter of a grain of arsenic dissolved in 8000 parts of any of the foregoing fluids.

ART. XXVI.—*Remarks on the Light of the Moon and of the Planets.* By JOHN LESLIE, Esq. Professor of Natural Philosophy in the University of Edinburgh, and Corresponding Member of the Royal Institute of France.

MODERN astronomers have generally assumed it as an evident truth, that all the planets and their satellites shine merely by the reflected light from the sun, the great source of heat and illumination. But this conclusion, however probable and consistent, is too hastily drawn, and would require some discussion to establish it. Are those celestial bodies rendered luminous by the rays of the sun, or by the emission of their own native light? Are the whole of the incident solar beams reflected by them, or a part only of those rays? Is the light simply reflected which the planets transmit to us, or is it rejected and dispersed in all directions, after having entered their surface? Or, lastly, is this light all emitted from their internal substance, in consequence of the absorption and calorific action of the sun's rays? These are so many curious questions, which it is necessary to solve before we can form a correct opinion on the subject.

1. Since the rays of light are darted in straight lines, their mutual divergency increases continually as they proceed, and consequently their illuminating power must be inversely proportional to the square of the distance from the point of emission. The quantity of light which the pupil of our eye can receive from any shining body will, therefore, be in the inverse duplicate ratio of the distance; but its visual magnitude, or the size of the image impressed on the retina, follows the same ratio; and hence the object must appear to us with exactly the same degree of brightness at every distance. Thus, a candle removed 50 yards from the eye, will seem just as brilliant as when placed only 10 yards from it; because, though the eye receives 25 times fewer luminous particles, these are concentrated into a space of impression likewise 25 times smaller.

This unexpected but important result is subject to no modification, except what may arise from the accidental loss of light during its passage through an intervening medium. No such obstruction, however, can occur in the celestial expanse, and, consequently, the proposition that a shining body, however remote, will appear always equally bright, is rigidly true in reference to the planets and fixed stars. Nor will it alter the effect, whether such bodies derive the radiating power from their own substance, or from the mere influence of external illumination. When the object lies so remote, however, that we can discern no longer its visual magnitude, which seems contracted into a mere lucid spot, we confound the intensity of its brightness with the quantity of light received from it. Supposing the fixed stars to have all the same constitution, their lustre, as judged by our unaided sight, would be in the direct ratio of the squares of their diameters, and the inverse ratio of the squares of their distances. But in the case of a planet, the application of a powerful telescope will expand the radiating point into a broad surface, and thus enable us to distinguish easily the density from the quantity of illumination. If Venus and Jupiter were constituted alike, and shone by their native light, the apparent lustre of the former, at the period of their superior conjunction with the sun, would be ten times less than that of the latter, though their relative brightness, as disclosed by the telescope, would continue the same. But neither of these inferences will agree with observation. On the other hand, if those two planets derived their luminous quality from the sun, Venus would have had only the twenty-fifth part of the lustre of Jupiter, though the brightness of her surface, when viewed by the telescope, would have been five times greater. This conclusion approaches nearer to the actual appearances.

But the different phases which the planets exhibit, according to the relative position of the sun, clearly prove that their light is merely dependent on the action of this great luminary. The obscuration which they periodically suffer, from the intervention of their satellites, indicates the same conclusion. The only question then is, to determine what changes the rays of light transmitted from the sun undergo at the surface of the planet.

8. It may be shewn, that if a planet were a perfect sphere, and reflected like a mirror the whole of the incident light, it would always exhibit a round image of the sun, extremely small, indeed, but equally bright as that luminary, and only varying in size according to its relative position. On this hypothesis, our planetary system would have appeared only a group of minute suns, and not betrayed their dependence by any distinction of phases. From the principles of catoptrics, we learn that each reflected ray would appear to flow from a virtual focus situate on the chord of each circular section of the planet, and at the distance of one-fourth of that chord behind the refringent surface. The image so formed by the reflexion of the light of the sun, subtends the same angle at the middle of the chord as that luminary itself. We may deduce this simple theorem, *That the diameter of a planet is to that of the solar image which it would reflect at its superior conjunction, as its distance from the sun, to the fourth part of his diameter.* Hence, at her opposition, Venus would have appeared a lucid circle, bright as the sun, but with only the 317th part of her actual diameter. In approaching to the inferior conjunction, this circle, still preserving the same brightness, would gradually shrink into a point. For a like reason, Jupiter, as a perfect mirror, would appear, at the superior conjunction, a sun of only the 2712th part of its diameter, and at the inferior conjunction with the 1797th part of its diameter. It may hence be calculated, that if Venus, at her opposition, were to reflect the whole of the light received from the sun, her illuminating power would not exceed the 3400 millionth part of that refulgent mass. By a similar computation, it will be found, that the quantity of light reflected by Jupiter at his superior conjunction, is only the 988 millionth part of the direct illumination of the sun, but at his inferior conjunction the 432 millionth part.

It is more interesting, however, to trace the operation of our moon. Had her surface acted as a perfect mirror, she would have appeared, after Change, a mere lucid spot, and gradually opened with the splendour of the sun, till it appeared at Full, to shine with a diameter equal to the 458th part of its real dimension. Supposing the moon, therefore, to send back the whole of the incident light, this would be still attenuated 210,000 times more than the solar beams. But the most singular result

of the supposition of a perfect reflexion is, that we should never have discovered the magnitude of the moon, surveyed her varied surface, or distinguished her successive phases. She would have had the appearance of only a very diminutive sun, alternately approaching to the earth and receding from it, but never advancing nearer than 458 times the distance of the real sun. The moon, therefore, does not strictly perform the office of a mirror, but scatters the incident solar beams in all directions, so that every portion of her surface looks more or less illuminated; and she displays, according as the side fronting the sun turns towards our equator, all the gradations from a thin crescent to the full orb.

3. The conclusion is hence incontrovertible, that if the planets and our moon shine by light borrowed from the sun, they must have a rough or matt surface, like that of paper or plaster. It is only through the medium, indeed, of such surfaces, that we are enabled at all to distinguish the natural colours of bodies. The particles of incident light are not reflected on their mere appulse, but suffered to penetrate the external crust, where they are mostly absorbed; while a certain portion of them is rejected, and again discharged. The rays thus remitted, after undergoing such internal secretion, serve to indicate the constitution of the substance in respect to colour. Even from polished bodies, besides the reflected light, there is always a large portion rejected after penetration. Thus a slab of polished marble partly represents the surrounding objects, and partly evolves its own native colours; but hold it in a position extremely oblique, and it will reflect almost the whole of the incident rays, and act like a colourless speculum. On the other hand, the colours of any stone are brought out, or rendered brighter, by wetting its surface, the film of water, by its refractive power, bending the rays inwards, and thus facilitating their penetration.

But the quantity of light sent back from a white matt surface, is in every case extremely small. According to Bouguer's experiments, paper and plaster discharge only the 150th part of the perpendicular rays, and a much less portion of the light when the incidence becomes oblique. If the moon appeared of an uniform whiteness, we could hardly reckon more than the 300th part of the solar beams to be again rejected and dispersed; but its surface being very irregular, and dimmed over by obscure spaces, this estimate may be reduced at least to the 500th part. Hence that

portion of the sun's light which the moon can remit to us, would be 500 × 210,000, or 105 million times diminished. Yet the quantity of attenuated light which we actually receive from the moon, exceeds this measure seven or eight hundred times. In fact, the light from the moon is equal, if not superior, to what could have reached us, if every point of her surface had performed the most perfect reflexion. Bouguer inferred from his observations, that it amounts to between the 250,000th and the 300,000th part of the direct light of the sun; and I found it, by the first application of my photometer, to approach to the 150,000th part, while we have seen that the portion of the solar beams which could ever be reflected from the Moon, exceeds not the 210,000th part of the total incidence. We are thus forced to conclude, that the light from the Sun, at the Moon's surface, is almost entirely absorbed, but exerts a power to cause the projection of a still greater quantity of luminous particles, which had lain combined with her substance. From some broad spaces, this latent light is feebly emitted, while, from certain lucid spots, it is discharged with extreme profusion. In short, we are compelled to admit, that the body of the Moon is really a *phosphorescent* * substance like the Bolognian stone, which, excited by the calorific illumination of the Sun, is made to shed its native light. Nor are instances wanted of analogous effects. The mineral just mentioned, or the sulphate of barytes, was first observed near two centuries ago, to shine spontaneously after being exposed to the rays of the Sun. Various calcined or incinerated

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* *Phosphoric* bodies are those which shine spontaneously in the dark. To this class belong phosphorus itself, and many substances in a certain stage of putrefaction, such as meat, fish, wood, &c. But the term *phosphorescent* is applied to substances which become lucid for a short time, on being exposed to any strong light. This property was first remarked in the famous Bolognian stone,—the *sulphate of barytes* or *heavy spar*,—which being suspected, from its ponderous nature, to contain silver, was roasted in contact with charcoal and other inflammable matters, for the purpose of extracting the ore. A similar preparation is made with calcined oyster-shells, which, being pounded and mixed with the third part of their weight of the flowers of sulphur, are rammed into a crucible, and subjected for half an hour to a red heat.

To the class of phosphorescent bodies likewise belong a numerous catalogue of minerals, which, when heated, shine with different colours in the dark. Most substances, as I have shewn in a former paper, are caused to emit various coloured light, by a powerful electrical excitement.

substances manifest the same property. A diamond held for a few minutes near the flame of an Argand lamp, and then carried into a dark room, will for a short time emit a vivid light. But this lustre is always white, whatever may be the colour of the rays absorbed, insomuch that the intervention of a red, green, or blue glass between the lamp and the diamond, will not alter the effect.

The Moon, examined by a good telescope, has absolutely the appearance of an incinerated mass. Some circular cavities and round protuberances shine with remarkable effulgence, while very wide and smooth plains seem to have nearly lost the phosphorescent property, and to have a brown or dark shade. The Moon being fifty times smaller than our globe, with only the seventieth part of its attractive power, has five-sevenths of the Earth's density, and is consequently about four times denser than water. She may then consist of stony matter similar to the terrestrial composition. But it would be rash to pursue the parallel any farther. No disposition is more fallacious than the propensity of mankind to personify all nature, and trace our image, habits, and operations in strange and unknown objects. Every spot of the universe that has been explored, teems with animation, —the land, the sea, and the air—are all tenanted by some kinds of living creatures. But what right have we to suppose that the inhabitants of the Moon have any relation or similitude to ourselves? There is no appearance of any water in the moon, nor of an atmosphere. But the former might be detected by its optical effects, and the latter by the occultation of the fixed stars. If any creatures, therefore, live in the moon, they must be very different from the occupiers of this Earth. We may perhaps be allowed to conjecture, that the surface of the Moon, apparently marked by numerous groups of extinct volcanos, is only recovering from its incinerated state, and advancing slowly into a condition fit for the growth of the vegetable tribes. A careful comparison of telescopic observations, after long intervals of time, might perhaps discover this progress of amelioration, and ascertain the gradual darkening of the surface, which must follow the decay of the lunar phosphorescence.

The ancient cosmologists and their poetical expounders, entertained a whimsical opinion, that the stars are fed by the hu-

midity attracted from the ocean; but that the Moon, being much nearer to our Earth, drew her nutriment only from the lakes, whose waters in exhaling, carried along with them portions of mud and other impurities, which partly encrusted and obscured her surface. Had the delineation been more precise, we could perhaps have marked the spread of such incrustation during the course of three thousand years. This mythological notion, however, is favourable to the hypothesis of the gradual change of the Moon's surface.

It deserves to be noticed, that the phosphorescent quality which we have thus been led, by a strict process of induction, to ascribe to the Moon, is not altogether a new proposition. Such an idea was started by Licetus, Professor of Philosophy at Bologna, soon after the discovery of the singular stone known by that name. But, not supported by any proofs, his opinion seems to have been soon neglected, and at length forgotten. It is barely mentioned in Riccioli's Great Collection *.

But recent discoveries in optics furnish another demonstration, that the moon shines by her native light. All rays reflected from glass or water, or generally from the surface of any body not of a metallic nature, become polarized. To acquire this modification, the light emitted directly from the Sun, a candle, or the fire, must undergo such a reflexion. But the rays of the Moon affect the very same disposition, and therefore must not have previously suffered reflexion at her surface. This ingenious and conclusive remark I owe to my celebrated friend M. Arago, who first communicated to me in conversation, at Paris in 1814. I have since repeatedly verified it, and have found that Venus and the other bright planets possess the same property.

The theory of lunar phosphorescence accords with the various phænomena. Three or four days after change, the very thin lucid crescent seems to embrace a dark grey circle, suffu-

* Heinrich, in his curious book on Phosphorescence, conjectures that the light of comets is of the same nature as that of the Bolognian stone, and hints that not only the Moon and the Earth, but all the planets, are more or less phosphorescent. Schröter, Harding, and other practical astronomers, from the phenomena exhibited by the Moon and Venus, were led to believe that the planets possessed a peculiar light, independent of that of the Sun. These details serve to complete the history of the idea of lunar phosphorescence first started by Licetus, and here brought forward by Professor Leslie, in so novel, interesting, and striking a manner.—Edit.

sed with a faint mistiness, vulgarly, but graphically, termed "The old moon in the new moon's arms." This appearance is commonly attributed by astronomers to reflected light from our earth. Were the whole of the incident solar beams sent back, the illuminating power would no doubt be thirteen times greater than that of the moon herself, and would hence amount to the 16,000th part of the direct influence of the sun. But the waters, which cover three-fourths of the surface of our globe, could reflect only the 55th part of the incident light; and the reflexion from the land would be far inferior. We may, therefore, infer, that the moon receives from earth less than the millionth part of the light sent to her from the sun. Such a faint illumination is hence quite insufficient to explain the cinereous aspect of the old moon, which seems to be the mere expiring glow of phosphorescence after it has long spent its force. This explication is confirmed by the existence of the fine silvery thread which appears to half inclose the ashy circle. If this extreme margin had been illumined by the earth, it would, from its obliquity, have appeared even fainter than the rest of the surface. But, being the last portion lighted up by the sun, it continues to glow for a short space after it has retired from his rays, and, presenting to the eye a very contracted front, or being *fore-shortened*, it appears the more vivid.

Were we to indulge imagination, we might suppose that the Moon has been a comet, which, chancing to come near the Earth, and to cross its path at right angles, was constrained to obey its predominant attraction, and henceforth to circulate about our planet. Its approximation, by raising stupendous tides, would have occasioned one of those overwhelming convulsions which this globe appears to have repeatedly suffered. But the new satellite would soon lose its fiery constitution, and conglomerate into a solid mass. In its subsequent progress, it will gradually assume a more earthly appearance. But when it shall have attained, in the succession of distant ages, the ultimate term of amelioration, the Moon will no longer cheer our nights by her soft and silvery beams; she will become dim and wane, and seem almost blotted from the blue vault of heaven. To our most distant posterity this prospect is indeed gloomy; but other changes will arise to renovate and embellish the great spectacle of the Universe.

Art. XXVII.—*List of Rare Plants which have Flowered in the Royal Botanic Garden at Edinburgh, during the last Three Months.* Communicated by Dr Graham, Regius Professor of Botany in the University of Edinburgh.

Ammobium alatum.

Cactus Melocactus.

——— nobilis.

Carica Papaya.

Cereus grandiflorus.

Chrysophyllum Cainito.

Combretum purpureum.

Fuchsia decussata.

The figure in the Botanical Magazine was taken from the first specimen which flowered in the Garden, and unfortunately, as this was less vigorous than others which have blossomed since, a very imperfect idea of the beauty of the species is given. In the last number of the Philosophical Journal, a hope was expressed that it would be found to rival Fuchsia coccinea: it now appears that it exceeds it far in splendor, the flowers and pedicel being 5 inches long, of a much more brilliant colour, the plant as free in flowering, and apparently as hardy. The leaves are of a more lively green, though perhaps their shape is scarcely so handsome.

Geranium Wallichianum.

Hibiscus speciosus.

Hippeastrum pulverulentum.

Brought from Rio Janeiro by Captain Graham in 1822.

Hyperanthera Moringa.

Jasminum revolutum.

In full flower now in the open air, and rising to the top of a wall 14 feet high.

Jatropha multifida.

Justicia spinosa.

Lobelia coronopifolia.

Nuttalia diversifolia.

Oxyanthus speciosus.

Paederota Ageria.

Philydrum lanuginosum.

Pistacia Terebinthus.

Potentilla splendens.

Don, Flor. Nepal. ined.

Psidium Cattleianum (*Purple-fruited Guava*).

Now bearing abundance of high flavoured fruit. It does not seem extravagant to expect, that this tree may come into cultivation, to add another luxury to the dessert. The fruit is not only very abundant and very rich, but in very various degrees of maturity upon the tree at the same time.

Reaumuria hypericoides.

Rondeletia hirta.

Roscoëa purpurea.

Plant procured from Lady Liston, who raised it from seed brought from Nepaul by Dr Govan in 1822.

Saxifraga cæspitosa.

Plants brought home by Dr Fisher from Captain Parry's 2d voyage.

Schizanthus porrigens.

In full flower now (August) in the open border. This species may be distinguished from *S. pinnatus* even in the seed-bed, by the seminal leaves being much shorter, as first remarked by Mr Macnab this spring. One plant in the green-house has already reached the height of five feet, though only beginning to come into flower.

Thalia dealbata.

Thysanotus junceus.

Art. XXVIII.—*Celestial Phenomena from October 1. 1824 to January 1. 1825, calculated for the Meridian of Edinburgh, Mean Time.* By Mr George Innes, Aberdeen.

The times are inserted according to the Civil reckoning, the day beginning at midnight.—The Conjunctions of the Moon with the Stars are given in *Right Ascension.*

October.

D	H	′	″	
2.	23	51	30	Inf. ☌ ☉ ☿
3.	4	39	2	Im. II. sat. ♃
7.	2	55	56	Im. I. sat. ♃
8.	3	30	57	○ Full Moon.
10.	2	3	3	Im. IV. sat. ♃
10.	4	13	25	Im. II. sat. ♃
11.	0	47	25	Im. III. sat. ♃
11.	4	16	33	Em. III. sat. ♃
12.	7	31	6	☌ ☽ ♄
14.	2	28	0	☌ ☽ η ♊
14.	4	49	25	Im. I. sat. ♃
14.	5	46	6	☌ ☽ μ ♊
15.	15	58	4	☾ Last Quarter.
16.	23	26	53	☌ ☽ ♃
18.	4	45	10	Im. III. sat. ♃
19.				☿ greatest elong.
19.	6	17	0	☌ ☿ γ ♏
19.	15	45	0	☌ ♂ θ Oph. ♂ 28′ N.
21.	0	49	0	☌ ☽ ☿
22.	7	35	13	● New Moon.
23.	1	11	13	Im. I. sat. ♃
23.	10	40	1	☉ enters ♏
23.	23	18	9	☌ ☽ ♀
26.	6	47	16	☌ ☽ ♂
27.	0	17	21	Em. IV. sat. ♃
27.	13	35	44	☌ ☽ 1 ν ♐
27.	18	24	38	☌ ☽ ♅
29.	17	50	15	☽ First Quarter.
30.	3	4	34	Im. I. sat. ♃

November.

D	H	′	″	
4.	1	14	21	Im. II. sat. ♃
6.	4	57	52	Im. I. sat. ♃
6.	19	14	20	○ Full Moon.
7.	23	26	10	Im. I. sat. ♃
8.	10	42	31	☌ ☽ ♄
10.	8	0	0	☌ ☽ η ♊
11.	3	49	16	Im. II. sat. ♃
11.	4	13	0	☌ ☽ ξ ♊
13.	9	25	30	☌ ☽ ♃
13.	23	50	12	☾ Last Quarter.
15.	1	19	25	Im. I. sat. ♃
16.	0	7	44	Em. III. sat. ♃
18.	6	24	18	Im. II. sat. ♃
20.	11	30	0	☌ ♂ ♅
20.	18	42	0	☌ ☽ ☿
20.	19	34	20	● New Moon.
21.	11	15	0	Sup. ☌ ☉ ☿
22.	3	12	40	Im. I. sat. ♃
22.	7	7	16	☉ enters ♐
23.	0	35	25	Im. III. sat. ♃
23.	4	6	16	Em. III. sat. ♃
23.	5	39	9	☌ ☽ ♀
24.	5	29	0	☌ ☽ ♅
24.	10	21	0	☌ ☽ ♂
26.	14	22	0	☍ ☉ ♄
28.	14	30	20	☽ First Quarter.
28.	22	17	36	Im. II. sat. ♃
29.	5	5	54	Im. I. sat. ♃
30.	4	33	5	Im. III. sat. ♃
30.	23	34	14	Im. I. sat. ♃

December.

D	H	′	″	
3.	20	45	0	☌ ♀ ♅
5.	14	56	25	☌ ☽ ♄
6.	0	53	6	Im. II. sat. ♃
6.	10	3	37	○ Full Moon.
7.	18	25	36	☌ ☽ μ ♊
8.	1	27	30	Im. I. sat. ♃
10.	15	26	49	☌ ☽ ♃
13.	3	28	46	Im. II. sat. ♃
13.	7	25	33	☾ Last Quarter.
15.	3	20	47	Im. I. sat. ♃
16.	1	57	56	Im. IV. sat. ♃
16.	6	24	36	Em. IV. sat. ♃
16.	21	49	6	Im. I. sat. ♃
20.	6	4	36	Im. II. sat. ♃
20.	10	25	45	● New Moon.
21.	12	42	17	☌ ☽ ο ♐
21.	17	14	40	☌ ☽ ♅
21.	19	48	5	☉ enters ♑
21.	20	0	0	☌ ☽ ☿
22.	5	14	8	Im. I. sat. ♃
23.	13	58	40	☌ ☽ ♀
23.	15	16	40	☌ ☽ ♂
23.	23	42	28	Im. I. sat. ♃
24.	23	50	0	☌ ♀ ♂, ♀ 43′ S. of ♂.
28.	12	5	31	☽ First Quarter.
28.	20	24	56	Im. III. sat. ♃
28.	23	56	53	Em. III. sat. ♃
30.	21	58	14	Im. II. sat. ♃
31.	1	35	53	Im. I. sat. ♃
31.				☿ greatest elong.

ART. XXIX.—*Proceedings of the Wernerian Natural History Society*. (Continued from p. 210.)

1824, *May* 1.—THE Secretary read a communication sent to the Society, entitled, "*Observations and Experiments on the Formation of Pearls.*" Also, notice of a substitute for cork in tropical climates, being the central part of the scape of the Agave vivipara, by the Reverend Lansdown Guilding of St Vincent's, with specimens of the prepared agave-cork. And likewise an "*Account of William Dempster, who accidentally swallowed a table-knife at Carlisle in November last*;" communicated by Dr Barnes of Carlisle. (This paper is printed in the present Number, p. 319. *et seq.*)

At the same meeting, Professor Jameson read a notice of the discovery of some fossil remains of a whale near the seat of Lord Dunmore, on the banks of the Forth, and at the distance of several hundred yards from the existing bed of the river; communicated by Mr A. Blackadder, Allan Park.

A specimen of the native dog of New Holland, presented to the Museum by His Excellency Sir Thomas Brisbane, was exhibited to the meeting, and described by Professor Jameson.

May 15.—At this meeting Professor Jameson communicated to the Society Mr A. Blackadder's observations on the alluvial strata of the Forth district, illustrated by a geological map of that portion of country.

Mr Witham of Larkington read a memoir on some peculiarities observable in the trap-rocks in the west and north-west of the counties of York, Durham, Westmoreland, and Northumberland.

The Secretary read a notice regarding the pernicious effects on fruit-trees, of a layer of bog-iron-ore immediately under the surface-soil, in Aberdeenshire, provincially termed *pan*; communicated by Mr Stevenson, civil engineer.

At the same meeting, Mr Deuchar, lecturer on chemistry, read a paper "*On the comparative merits of the theories of Galvanic Action.*" And Mr F. A. Parry exhibited a remarkable stalagmite or deposition, found at the bottom of one of the tanks of lime-water through which the coal-gas is passed, with a view to its purification, at the Edinburgh Gas Works.

The meetings of the Society were then adjourned till November next.

Art. XXX.—SCIENTIFIC INTELLIGENCE.

ASTRONOMY.

1. *Astronomical Observations by Dr Olbers.*—Comets that finish their course in a short period, do not appear to differ in form, shape, or nature, from those that require a long time to complete their motion around the sun. Their external aspect does not agree with that of the newly discovered planets. Although the late Schröter pretended that he observed a considerable nebulous envelope around the asteroids Ceres, Pallas and Juno, Olbers is not convinced of its existence, and is rather inclined to consider the appearance as a deception, arising from the imperfect nature of the object-glass, as he could not discover any such nebulosity, although he used the most perfect dioptric instruments. Olbers considers the greater number of *double stars* to be *suns*, which are not only apparently but also in reality comparatively near to each other, and that describe regular orbits around their common centre of gravity. He says that he has never observed any nebulous covering in Gassendus, in Possidonius, or indeed in any other part of the moon, nor in general any change in the lunar spots, which could not be referred to the different angles of illumination, and the relations of libration. Nor can he give any opinion as to the traversing furrows in the circular mountains; and he adds, "I must acknowledge, that, in regard to the beds of rivers, buildings, &c. said to have been observed in the moon, I fear that a lively imagination may have frequently seen more than is shewn by the telescope."

METEOROLOGY.

2. *Fall of Meteoric Stones.*—Many meteoric stones fell in the vicinity of Arenazzo, a village in the Papal dominions, in the month of March. The largest stone weighed 12 lb. Before the fall, loud thunder was heard. The large aërolite, weighing 12 lb., was carried to Bologna, where it is preserved in the Observatory.

3. *Account of a St Elm's Fire seen in Poland.*—Captain Bourdet gives an account of an electrical appearance he ob-

served in Poland, in the month of December 1806. The winter was remarkably mild, no snow had fallen, but storms were frequent. One evening about 9 o'clock, after a violent gust of wind, the night became so dark that riders could no longer see even the heads of their horses, and so violent a storm arose that the horses were forced to halt; but their ears became speedily luminous at the tops, as also all the long hair of the body, with exception of that of the mane and tail. All the metallic ends of the harness became luminous, as if they were covered with a swarm of luminous worms. The whiskers of M. Bourdet, and that of the other cannoneers, also shone; but neither the eyebrows nor hair became luminous. This appearance continued as long as the gust of wind; that is, about three or four minutes. As soon as the wind ceased, the luminous appearances vanished, and a violent shower of rain fell.

4. *Remarkable Whirlwind.*—On the 6th July 1823, at 1 h. 35 m. P. M., in the plain of Assonval, a village situated six leagues WSW. of St Omer, and an equal distance from Boulogne, the labourers were obliged to quit their work on account of the darkness, and from the dread of a tempest with which they were threatened. Clouds coming from different points rapidly collected over the plain, and presently united so as to form a single vast cloud which covered the whole horizon. A moment after, there was seen to descend from this cloud a dense vapour, having the bluish colour of sulphur in a state of combustion: it formed a reversed cone, the base of which was connected with the cloud. The lower part of the cone, which descended upon the earth, turning round with considerable quickness, presently formed an oblong mass of about 30 feet, detached from the cloud. It rose making a noise similar to that of a large bomb-shell on bursting, and leaving in the earth a pit in the form of a circular basin, from 20 to 25 feet in circumference, and from 3 to 4 in depth at its middle. At the distance of scarcely a hundred yards from the point of departure, and directing its course from west to east, the whirlwind broke down the hedge of a dwelling house, overturned a barn, and gave the house, which was more solidly built, a shock, which the farmer compared to that of an earthquake. It had, in breaking the hedge, torn asunder and carried off the tops of the strongest

trees; from twenty to thirty trees were overturned and laid in different directions, in such a manner as to prove that the motion was rotatory; others were lifted up and hung, together with several tops, upon the higher branches of trees from 60 to 70 feet high. After this, the whirlwind traversed a distance of two leagues without touching the ground, carrying with it very large branches of trees which it threw out on all sides with noise. Having come to the elevated point of the wood of Fauquembergue, it tore off the heads of several oaks, which it was seen to carry along with it above the village of Vendome, situate at the foot of the hill on the east side of the wood. It did no other injury in this commune than that of lifting with its root a very large sycamore, in a meadow belonging to M. Degrosseillers; the tree was found at the distance of 600 yards. Continuing its course in the manner of a ball which strikes the earth and rebounds again, the whirlwind proceeded to the village of Audinctan, where it threw down the roofs of three houses and lifted up several trees; among others five elms of very large size, all growing from the same root. On leaving the valley in which these two last villages are situate, the whirlwind rose upon a mountain, named *de Capelle*. Several country people who were at work, saw with terror this extraordinary phenomenon pass over their dwellings. They were immediately struck with a sense of the danger, but had only time, in order to avoid it, to lie down and keep fast hold of their plowing instruments. They remarked with astonishment that their horses were disquieted, but did not take fright. The sock of one of their ploughs was buried in the earth deep enough to resist the efforts of three horses, and to prevent its being injured, they employed a pick-axe in extracting it. It was from these labourers, who were so situate upon the mountain as to see the whirlwind arrive and continue its course, that its form and size were learned, as well as the supposed elements which may have entered into its composition. The form was oval, the length appeared to them about 30 feet, the other diameter might have been 20. It turned round in its progress such a manner as to present each of its faces to all the points of the horizon. There issued occasionally from its centre balls of fire, and often also globes of sulphureous vapours. Both of them cast out, in different directions, branches which

the meteor had drawn along with it from a distance. The noise which it made in its rapid progress was similar to that of a heavy carriage rattling at full speed upon a stone pavement. An explosion, like the report of a musket, was heard at each eruption of fire or vapour; the wind, which was impetuous, joined a fearful whizzing to this noise. After having torn up the earth and carried off whatever offered resistance in one place, it rose from the ground, to proceed to another, a league and sometimes two leagues distant, to recommence its ravages. Thus, on leaving Mount Capelle, always following the same direction, it lighted next at Hernies St Julien, at the distance of a league from the mountain, where it carried off several hay-cocks, and a number of trees. From this village to Witernestie, over an interval of three leagues, it committed no ravages worthy of mention, there having been only observed upon the mountain which separates Hernies from Etree-Blanche, a furrow of the breadth of thirty yards, in which the corn was destroyed, over an extent of thirty acres of land, situated at the summit. From hence it penetrated into the valley of Witernestie and Lambre. In the former of these villages, which consisted of forty dwelling houses, eight only were left uninjured; thirty-two houses with their barns were overturned, and an enormous quantity of trees thrown down, torn asunder, and carried to a great distance. It was observed at Witernestie, that the roofs and walls of the houses were laid in a diverging manner, from within outwards. The injury done was not less considerable at Lambre. Several persons observed perfectly the turning motion of the meteor, its sulphury brown colour, and the centre of flame from which proceeded blazes of bituminous vapour. The trees surrounding the church were broken and torn up by the roots; the wall and roof of the curate's house carried off, and eighteen houses, the greater number built of brick, levelled to their foundations, with the extraordinary phenomenon of the walls being separated by having been thrown outwards. A fortunate circumstance, in the midst of this great calamity, was that nobody lost his life, even in the two last villages; a single individual at Witernestie was severely wounded in the arm by a beam. On leaving the village of Lambre, the whirlwind divided into two; one portion was dissipated in the air; the other, which now appeared only as a cloud

driven by a boisterous wind coming from the north-west, proceeded to Lillers, a town three leagues distant from Lambres, where it broke and uprooted nearly two hundred trees in the beautiful grounds of M. Defoulers; after which it was dissipated in its turn. At three o'clock, the weather was calm, the sky almost entirely clear, and the thunder, which had been heard from all points of the horizon, ended at the same time with the whirlwind. The evening and night were very beautiful.—*Bulletin Universel.*

GEOGRAPHY.

5. *Height of Mount Ætna.*—Captain Smyth, in his lately published "Memoir of Sicily," informs us, that, according to his measurements, Mount Ætna is 10,874 feet above the level of the sea, and not 12,000 feet, the height usually given to it. Professor Schouw, in his "L'ultima cruzione dell 'Etna, descrita in una lettera diretta al Cavaliere J. J. Alberto de Schœnberg, dal Dr J. Schouw, Giornale Enciclopedico, Agosto 1819," gives nearly the same height to the mountain; it being, according to his measurement, 10,484 French feet above the level of the sea.

HYDROGRAPHY.

6. *Observations with regard to the presumed Diminution of the Level of the Baltic Sea, by N. Bruncrona; with Remarks on the same subject, by C. P. Hollström.*—The phenomenon of the diminution of the water, or, to speak more correctly, of the lowering of the level of the sea in the Baltic, has long engaged the attention of Swedish naturalists, some of whom are satisfied as to its existence, while others deny it altogether. The latter have, on their side, the difficulty of admitting that a body of water, internal, it is true, but communicating with the ocean by three openings, could have any other level than that of the ocean itself; but, to this objection, their opponents reply by facts, that is, by the comparison of the present level of the Baltic, with that which it must have had at periods more or less remote, judging by the marks impressed, either by the operation of natural causes, or by the hand of man, upon the rocks which project from the midst of the waters. A circumstance not less remarkable than the phenomenon itself, is, that the fall of the level seems to have

followed different laws in different parts of the Baltic. In the Gulf of Bothnia, it has been estimated at four feet in the century. From this it diminishes southward, so as to be limited to two feet in the century, upon the coast of Calmar; and there is reason to think that no diminution at all has taken place in the southern part of this sea; and that, if any in its eastern part, it must at least be very small.—Admitting the reality of the phenomenon such as we have above announced it, and which appears to be now admitted by the greater number of the Swedish naturalists, there remain many important questions to be answered. 1*st*, What is the total quantity of the diminution produced in the level of the Baltic, at a given point, and in a given period of time? There still prevails much uncertainty on this subject; and anomalies exist, which, in points very near upon each other, are in the relation of one to four, and even of one to twelve. 2*dly*, Has this lowering of the Baltic taken place in a uniform manner? Are its variations, if any there be, regular or irregular, and subordinate to appreciable causes, such, for example, as is the case with the state of the atmosphere? 3*dly*, Is the diminution observed on proceeding from north to south, and which becomes at length evanescent, proportionate to the latitudes, or subjected to other laws. Such must be the object of the further inquiries of observers; for the establishment of the fact is of little importance, without a knowledge of the laws by which it has been regulated. But this is a difficult task to perform. It is not, in fact, sufficient to form a correct estimate of the action of local changes which may have affected certain points, without influencing the general mass of water in the Baltic; and, with this view, to choose, as places for marking, the most isolated rocks, the most remote from the shores, and situated in the deepest water. It is further necessary to be assured, that the comparative observations have been made when the sea was in a perfectly similar state. In fact, the Baltic, without having any ebbing and flowing, is subject to a balancing of its waters, dependent upon the direction and power of the winds. It rises when strong north-west winds, of long continuance, have driven in the waters of the Northern Ocean, by the Cattegat, the Sound, and the Belt. It falls, on the other hand, after a continuance of winds blowing in a direction calculated to propel its waters to-

ward these openings. The difference of level between the two extremes may amount to several feet. It is obvious that, were a line drawn, by hypothesis, when the Baltic was at its greatest elevation, and were its level measured at the same point when it is lowest, it would seem lowered much beyond what it would have been found, had the two corresponding observations been made when the level was at its mean height. It is therefore this mean height which it is of importance to determine, in either case,—a task which, without doubt, presents great difficulties, but which is absolutely necessary to insure the correctness of the observations.—Such is the substance of M. Hællström's opinion on the subject, as stated in his memoir. It is to be regretted, that the precautions of which he speaks, had not been taken into consideration by M. Bruncrona, as they would have rendered the observations more useful, which he made in 1820, at thirty different points, by means of the pilots under his command, as well as the care he has taken to have lines engraved upon various rocks to mark the level of the sea, and answer as so many points of reference to future observers.—M. Hællström passes rapidly, and with a sort of contempt over an idea received among the inhabitants of the small islands on the coast of Bothnia, and which, to be the opinion of these ignorant and perhaps incorrect observers, has not been entirely rejected by some philosophers; namely, that the level of the surface of the sea does not fall, but that the bottom is gradually raised, at least in the north of Sweden. The author throws ridicule upon this opinion, by saying, that, since this effect is not observed upon the shores of the Islands of Gothland and Oeland, which are calcareous or sandy, but only upon those of the Gulf of Bothnia, which are composed of gneiss abounding in felspar, we must attribute to this latter rock an aptitude for rising which the others do not possess. It belongs to the Neptunists to see whether this objection appear to them sufficient to make them change their opinion.—*Bulletin Universel.*

CHEMISTRY.

7. *On Metallic Titanium.*—Dr Walckner of Freyberg, in the Breisgaw, has lately described cubical crystals of metallic titanium which he observed in iron-slags from the Upperland of

Baden. The slag was from a furnace where pea-ore (bohnerz) was smelted, and analysis proved that this ore of iron contained a minute portion of titanium. Dr Wollaston, as is well known, was the first who described these cubes. They occur in the slags of iron-forges in Wales, in those at Bradford in Yorkshire, Alfreton in Derbyshire, at Pointypool in Monmouthshire, in Clydesdale in this country, and we have no doubt will be met with in many other places where our common iron-ores are smelted.

8. *Selenium in Volcanic Sulphur and Iron Pyrites.*—In our last Number we stated that Stromeyer had discovered selenium in the volcanic sulphur of Lipari; we may now add, that this sulphuret is the red substance found in the Lipari Islands, which used to be considered as sulphur coloured by oxide of iron. Selenium occurs also in some varieties of iron pyrites.

9. *Bitumen in Native Sulphur.*—It would appear from a series of experiments by Vauquelin, in the *Annales de Chimie et de Physique*, vol. xxv, that most native sulphur contains a small portion of bitumen.

MINERALOGY AND GEOGNOSY.

10. *Jeffersonite not a new species.*—Dr Troost, in an interesting memoir on the Augites (pyroxenes) of the United States of America, in the first part of the third volume of the Journal of the Academy of Natural Sciences of Philadelphia, shews that the mineral described as a new species under the name of *Jeffersonite* by Professor Keating, is a true Augite. Keating, he remarks, was led into error, by mistaking the base of the primitive form for one of its sides. Dr Troost names this variety *Foliated Augite.*

11. *Maclureite not a new Species.*—Dr Torrey and Mr Nuttall, two active and intelligent American naturalists, some time ago named a supposed new mineral from Brandywine, *Maclureite*, in honour of the distinguished President of the Academy of Sciences of Philadelphia. In a memoir published in the first part of the third volume of the Journal of Natural Sciences of

Philadelphia, Mr L. Vanuxem, refers it to the Augite species, and names it *Lamellar Augite*.

12. *Serpentine a distinct mineral species.*—Werner long ago arranged Serpentine as a distinct species of simple mineral; but Haüy viewed it as a mountain rock, and rejected it from the mineral system. In the Transactions of the Royal Society of Edinburgh, mention is made of crystallised serpentine; and Vanuxem, in the Memoirs of the Academy of Sciences, adopts the opinion of Werner, and enumerates and describes the following subspecies of it, under the name Marmolite, viz. 1. Lamellar Marmolite or Serpentine; 2. Compact translucent Marmolite or Precious Serpentine; 3. Common Marmolite or Serpentine.

13. *Discovery of Jenite in America.*—Dr Troost, on examining a series of specimens of minerals collected in Rhode Island by Major Ware, found in a granular quartz numerous crystals of Jenite, or Lievrite, as it is named by Werner.

14. *Lignite, or Brown Coal, in Coarse Marine-Limestone (Calcaire grossière.)*—Desnoyers has observed the following arrangement at Vaugirerd, near Paris. The lowest rock is plastic clay with lignite, containing paludinæ, helices, &c.; above this, chloritic and sandy coarse marine limestone, then the proper marine limestone; above the well-known bed containing impressions of plants, there are small nests of lignite; then the bed with lucinæ, and two series of short strata of siliceous limestone, with earths, and then a bed of marly limestone, with cerithia, modiolæ, planorbes, lymneæ, &c.; then a bed of plastic clay, with the same mixture of marine and fresh-water shells, and with spathose gypseous veins, and perhaps with nodules of compact phosphat of lime; then a bed of marl, like the preceding under the clay, with the same shells; then alternations of limestone, more or less siliceous, with yellowish or whitish clayey marl. These last are admitted by Brongniart as analogous to the siliceous limestone of Champigny; but this plastic clay with shells, forming a bed of from 2 to 8 feet thick, shews that Brongniart's idea of alternations of fresh and salt water lakes is inconsistent with nature. We have always considered this opinion as questionable, and are disposed to consider as more

plausible the opinion of Boué, that fresh-water deposits begin only with the upper fresh water limestone,—that the plastic clay, and gypsum, with bones, have been deposited under a salt or saltish water, or in lagoons, and that the fresh water and terrestrial fossil organic remains have been carried thither by rivers. According to some authors, the sea was as high above the Alps at the time of the formation of the Paris basin, as when the magnesian limestone was deposited; while others maintain, that the waters of the globe have subsided at two different epochs, those of the formation of porphyry, and the formation of basalt. —*Note communicated to the Editor.*

15. *Necker on the Dikes of Somma.*—Necker, of Geneva, has published, in Mem. Soc. Phys. Gen. vol. 2, an interesting account of the Lava Dikes of Somma. The first notice of these dikes was given by Sir James Hall in the Edinburgh Transactions, Vol. iii.

BOTANY.

16. *Botanical Excursions to the Scottish Mountains in June and July* 1824.—In the course of an excursion to the Breadalbane Mountains in June last, two plants were added to the British Flora, *Arenaria rubella* (Alsine rubella, Wahl. Lapp. 128. t. 6.) and *Hypnum trifarium*, Weber and Mohr. The first was discovered on the same day by Dr Greville and Mr Earle, who found it on different parts of Craig Challiach. A single specimen was subsequently detected on Ben Lawers. This plant is highly interesting, as it has hitherto been only known as a native of Lapland and the Arctic Regions, whence it was brought by the gentlemen who accompanied Captain Parry on his last voyage. The second new plant is *Hypnum trifarium*, which Dr Greville had the good fortune to discover on several mountains of the same range. Dr Hooker and Dr Greville consider this moss as unquestionably distinct from *Hypnum stramineum*, to which it has been united by some foreign muscologists. The leaves are perfectly regularly and beautifully trifarious. Several rare plants were also collected, as *Saxifraga cernua*; *S. nivalis*; and *Salix rupestris*. *S. vacciniifolia* and other rare species (by Dr Hooker). *Dryas octopetala*; *Carex atrata*; *C. capillaris*; *Splachnum vasculosum*; *S. tenue*;

S. mnioides; *S. sphæricum*; *Hypnum moniliforme*; *Encalypta alpina*, &c.

In the last week in July, Dr Hooker, Mr Burchell, and Dr Greville, made an excursion to the Mountains of Clova in Angus-shire. They were accompanied by Mr Drummond of Forfar, an excellent practical botanist, who has explored these mountains in repeated journeys, and been extremely fortunate, especially in discovering several of the rare plants with which that most active and acute of Scottish botanists, Mr George Don, had enriched the Flora of his country *. The following list contains the rarer plants gathered in this excursion: *Veronica alpina*, L.; *V. serpyllifolia*, var. *humifusa*; *Alopecurus alpinus*, Sm.; *Poa flexuosa*, Sm.; *P. alpina*, L.; *Luzula arcuata*, Hook. Fl. Lond. new series, t. 151., (Juncus arcuatus Wahl.) *Epilobium alsinifolium*, Vill.; *Pyrola rotundifolia*, L.; *P. minor*, L.; *P. secunda*, L.; *Saxifraga rivularis*, L.; *S. nivalis*, L.; *Spergula saginoides*, L.; *S. subulata*, Sm.; *Linnæa borealis*, L.; *Astragalus campestris*, L.; *Hieracium alpinum*, L.; *H. Halleri*, D. C.; *H. Lawsoni*, Vill.; *Sonchus cæruleus*, (Engl. Bot. t. 2425., S. alpinus, Willd.); *Serratula alpina*, L.; *Erigeron alpinum*, L.; *Carex rariflora*, Sm.; *Betula nana*, L.; *Salix glauca*, L.; *S. arenaria*, L; *S. lanata*, L; *S. myrsinites*, L.; (E. B. t. 1360.) *S. rupestris*, Don; *S. phylicifolia*, L.; &c.—In the class Cryptogamia, the following rarities occurred, *Gymnostomum Griffithianum*, Sm.; *Grimmia unicolor*, Hook. Grev. Crypt. Fl. t. 123.; *G. torquata*, Hornsch. MSS.; *Trichostomum funale*, Schwægr.; *Dicranum fulvellum*, Sm. (Weissia?); *D. strumiferum*, Sm.; *Didymodon glaucescens*, Grev. Crypt. Fl. t. 127.; *Splachnum vasculosum*, Hedw.; *S. tenue*, Dicks; *Orthotrichum speciosum*, Nees; *O. Drummondii*, Hook. and Grev. Crypt. Fl. t. 115.; *Daltonia heteromalla*,

* Mr Drummond is at present engaged in publishing a collection of dried specimens of Scottish Mosses. The first volume has appeared in 4to, entitled, "Drummond's Musci Scotici." It contains 100 species, arranged like those published by Drs Mougeot and Nestler in Germany. The present volume contains, among others, the following new or rare species; *Bartramia ithyphylla*, *Conostomum boreale*, *Daltonia heteromalla*, *Dicranum crispum*, *Didymodon inclinatum*, *Diplocomium foliosum*, *Gymnostomum Griffithianum*, *G. Lapponicum*, *Hypnum Crista-castrensis*, *H. stramineum*, *Orthotrichum Drummondii*, *O. rupincola*, *Trichostomum ellipticum*, *Weissia nigrita*. The price of each volume is L. 1.

Hook. and Tayl. (This was found in the grounds of Charles Lyell, Esq. of Kinordy.) *Hypnum silesianum*, Pal. de Beauv. *H. Crista-castrensis*, L.; *H. flagellare*, Dicks; *H. molle*, Dicks; *Jungermannia setiformis*, Mohr.; *J. Doniana*, Hook.; *J. albescens*, Hook.; *J. orcadensis*, Hook.; *J. Taylori*, Hook.; *Lecidea decipiens*, Ach., &c.

ZOOLOGY.

17. *Preserving of Birds, &c.*—Sir John Sinclair has communicated to us the following notice: Mr Temminck, Director of the Dutch Museum, has, for many years, made use of no other means of saving preserved birds and quadrupeds from the attacks of minute insects, than placing a small wooden basin, containing tallow, *in each case*, which he finds to be more effectual than either camphor or Russia leather.

18. *Further particulars in regard to the Fossil Whale of Dunmore.*—"Mr Monro having informed me that you had been making inquiry upon the subject, I went over to Dunmore, and procured the following particulars:—In a line almost directly north from the beautiful Gothic mansion lately erected by the Right Honourable the Earl of Dunmore, in Dunmore Park, and about 600 feet from the River Forth, and where the soil is the usual alluvial deposite found along the banks of the river, the waste-drain of a field on the south side of the new line of the *Kersie road* from Stirling to Linlithgow, was, in the month of August last, deepened to about 4½ feet, when the workmen came upon a substance, which at first they took for the trunk of a tree; but, on a little examination, its cellular texture soon convinced them that it was the bone of some huge animal. From the discovery of the Airthrey whale, a considerable interest was excited, and his Lordship, in the most attentive manner, gave directions that the greatest care should be observed when his workmen encountered a bone. During the operations, and to the extent they had occasion to be carried, a number of very entire bones, chiefly vertebræ, were dug out, and deposited in the offices at Dunmore House. In consequence of your desire, I last week visited the spot, and although the fields beneath which it is buried, are at present under a crop of wheat, I procured assistance, and easily succeeded, by uncovering a small portion of the soil, in procuring several pretty large bones.

These are now forwarded to you, and from one of them, apparently, a lumbar vertebra, an idea may be formed of the general state in which the bones are. They are found at the depth of about two feet from the surface. When first taken up, those of open structure are apt to separate into pieces, but upon exposure to the atmosphere, soon acquire considerable firmness; and the gardener at Dunmore assured me that a large piece had been, some time ago, lifted, which was not only of a very white colour, but so hard that he used a plane in reducing it to a regular shape. The skeleton is undoubtedly that of a whale, and it has been traced to the distance of nearly 80 feet from the drain which borders the road; although the bones may not be quite close together, yet it may be presumed that the animal measured from 85 to 90 feet. The tail lies in a north-easterly direction from the head, and just in such a situation as it may be supposed the animal would acquire, had it run aground in coming up the river. By a measurement, it has been ascertained, that the place where the remains lie, is between 23 and 24 feet higher than the highest tide of the Frith at present. The soil in which they occur is a stiff clay, which has been found near the spot to extend to a depth of 27 feet, resting on freestone. Stags' horns are occasionally found imbedded in the same clayey stratum. A fine pair, in a very perfect state, were got, many years ago, which I believe are still preserved at Carronhall. From the proximity of the situation of the skeleton to the river, no building or other artificial monument remains to determine, how long, within the bounded limits of historical record, the land has been deserted by the sea. Many years ago, an iron anchor was dug up a little to the south-east of it. The fleuks were much decayed, but the beam, which was of a rude square form, with an iron ring, was tolerably perfect. It hung many years in the old tower near Dunmore, but was at length stolen. Dunmore Moss extends a great way to the south-west, and in it, at about 300 yards from the skirts of the wood, are found the roots of large oaks. From all these facts, there seems but little doubt, that, under the same circumstances as those attending the fossil remains at Airthrey (nearly the like height above high-water, and having the same soil for its matrix, &c.) this animal was stranded where it now lies, about

the same period as that animal: and I am the more convinced of my original idea, that a number of them must have been stranded in coming up the Forth at the same time, and that many more of the flock remain to be discovered, where chance may direct the improvements on neighbouring estates. The lovers of natural history are under great obligations to his Lordship, for he has, in his usual liberal manner, determined to prosecute the investigation, and has requested me to convey to you his wishes, and the pleasure he shall have of complying with every thing which may be deemed necessary relative to the destiny of the whale. When the crop is removed, I am commanded to assure you, that these very interesting remains are entirely at your disposal."—*Letter from Mr Keddoch to Professor Jameson.*

19. *Fossil Elephant's Tooth found in Cheshire.*—The grinder of a fossil elephant has been lately found in a *marl-pit*, near Sandbach, in Cheshire, and is now deposited in the Museum of the Royal Institution of Liverpool.

20. *Sangiovanni on the Regeneration of Earth-worms.*—Dr G. Sangiovanni of Naples, an intelligent comparative anatomist, laid before the Academy of Sciences of Naples, a very interesting detail of experiments he made on the common earth-worm (Lumbricus terrestris), from which it appears, that he cut three of these animals into six, and that, in the course of a few months, these grew so as to form six perfect worms.

21. *The Trumpeter-Bird, a true Ventriloquist.*—Dr Traill informs us, that one of his friends in Liverpool has a living specimen of the Psophia crepitans, the *Trumpeter* of English ornithologists. It is, he says, a very social bird, following every individual of the family, and allowing itself to be caressed. The noise it makes has been supposed by some naturalists to have proceeded from the *anus*; but Dr Traill has ascertained that the bird is a genuine *ventriloquist*, of the most perfect sort. In this specimen, too, the bill is remarkable, by having the lower mandible about one quarter of an inch longer than the upper. This seems to be the usual form of the bill, but may easily be lost in dead specimens, or in stuffed skins.—Some of the frog tribe are also remarkable for their ventriloquial powers.

22. *Mallah of Miana, a venomous insect found in Persia.*—Several recent travels in Persia present a frightful picture of the effects of the puncture of a parasitical insect which occurs in that country, and more especially at Miana, a small town on the route from Tauris to Teheran. According to M. Dupré, this town is surrounded by rivers, which render a residence there unsupportable in summer, from the quantity of troublesome insects which are generated, and especially the Mallah, a sort of tick, which produces death unless the person punctured by it carefully avoids animal food, and acid or fermented liquors, and makes use of sugar, which is the only effectual remedy. It shuns the light, and does not occur in houses newly built. M. M. Kotzebue, agrees with Dupré in his account of the fatal effects of its puncture. He says that it avoids the open day, keeping itself concealed in the holes of old walls, and that its poison assumes more activity during the heats of summer. He remarks, that the effects of its puncture upon the natives, are trifling compared with those experienced by strangers, and adduces several instances of death, preceded by delirium and intolerable pain produced by it.—Morier and other travellers are of opinion that this formidable insect is not a bug or tick; and as it was of importance to determine the truth, M. Fischer of Waldheim obtained specimens through the Russian Ambassador in Persia, M. de Mazarovitch, and Mr Calley, an English gentleman resident in the country. He determines it to be a tick, or one of those parasitic insects belonging to the family of Acaridæ, such as occur in all countries upon dogs, oxen, and other animals.—M. Larveille was the first entomologist who distinguished the genus *ixodes*, and that of *argas*, two of the most noxious to man and the other animals which they infest, and to the latter of which the mallah belongs. They are very remarkable for the form of their suckers, and for the structure of their feet, which enables them to stick close to the skin. The genus Argas was formed upon the *Acarus reflexus* of Fabricius; the body is very flat, and of an oval form; the sucker, which is situated under the body, is not contained in a sheath formed by the palpi. There occurs in Italy, and the south of France, especially upon pigeons, an argas, bordered all round with a pale yellow colour, with darker shades. This species is figured by

M. Coquebert. The *Argas persicus*, the insect which has given rise to the exaggerated accounts of which we have taken notice above, is figured by M. Fischer in his notice. It is of the size of a bug, of a clear blood red colour, the back covered with white elevated points, the feet pale. It has a slight notch before, and beneath the sucker occurs with small palpi, attenuated toward their root. The eight feet have six joints, and are of a pale yellow colour; the terminal joint has two very small and hooked claws, at its curvature.—It is obvious, as Mr Fischer observes, that the accidents described by the travellers above alluded to, have had no connection with the puncture of this insect, but have arisen from a sort of malignant pustule, or anthrax, caused by the intensity of the heat in summer in this marshy country, especially in strangers. The symptoms enumerated agree precisely with those manifested by the disease in other countries, in the south of France for example, where it is equally attributed to the puncture of a venomous insect. The *Furia infernalis*, which Linnæus had been induced, from similar prejudices, to admit as the cause of a sort of gangrenous pustule, is no doubt in the same predicament; the true cause of this disease in Sweden, being to be sought for in the heats of summer exerting their influence upon a marshy country.—*Journal de Pharmacie*, No. 5. May 1824.

COMPARATIVE ANATOMY.

23. *On the Nervous System of Avertebral Animals.*—Professor S. delle Chiaje of Naples maintains, from numerous experiments and observations, *that the so-called nervous system in molluscous and other avertebral animals, is an absorbent system.* The following, besides other statements, are given in proof of the accuracy of this opinion. 1. The ganglia of the so-called nervous system are of different colours in different parts of the body, being yellowish, orange, whitish, and reddish. 2. The nerves occur most abundantly in the vicinity of the organs of digestion; indeed, in some manner surrounding them. 3. He very frequently injected all of these nerves, and observed in some injections the mercury of the injection to pass from the nerves into the veins, and in others from the veins into the nerves. From these facts it follows, either that the system is

an absorbent one, or, if nervous, that the nerves are hollow. Poli, in his well known work, when treating of the Pinna nobilis, conjectures that these parts are very probably absorbent vessels. Professor Chiaje, when he commenced his anatomical investigation of the avertebral animals, believed with Cuvier, and most anatomists, that the organs in question were true nerves; and it was not until after numerous injections, that he could admit the plausibility of Poli's opinion. These statements are so remarkable, that we hope they will ere long be brought to the test of experiment by our comparative anatomists.

24. *On the Semidecussation of the Optic Nerves.*—In a letter to us from a distinguished philosopher in Germany, the following remarks occur: "I do not understand how it happens that the labours of the Germans, and even of other nations, in comparative anatomy, are so little known in England. Many observations and opinions, which are considered as new in your country, have been long known to us in Germany. In proof of this I may mention, that, in the thirty-fourth number of the London Journal of Science, there is an extract from a memoir on the "*Semidecussation of the Optic Nerves*," in which the illustrious author, from a pathological appearance he observed infers a partial crossing of the optic nerves, without appearing to know, not only that many authors, from similar grounds, have come to the same conclusion, but also, that this kind of crossing had been observed in the eye of the human species by Vicq d'Azyr, Caldani, the brothers Wentzel and Chiasmon, and by G. R. Treviranus in the eye of the Simia Aygula. (*Vide Verm. Schriftin, von G. R. & L C. Treviranus*, Th. iii. p. 168.)"

25. *Discovery of a particular Organic System in the Cephalopoda.*—Dr G. Sangiovanni of Naples has discovered, in cephalopodous animals, a new organic system, which he names *Cromofero*, or *Colorifero*. An account of his observations has been lately laid before the Royal Academy of Science of Naples, and an abstract of them published in the Salzburg Journal. The following are the principal facts contained in that abstract: The whole surface of the cephalopoda, and particularly that

of the upper part and sides of the body, is strewed over with numberless small coloured grains or vesicles, nearly the size of a grain of sand. Even the iris of these mollusca is richly ornamented with coloured vesicles, which communicate a new beauty to the shining and variegated metallic lustre with which it is painted. Each of these vesicles has but a single colour. The principal colours in the species of cephalopoda, that occur in the Mediterranean, are yellow, rose-red, chesnut-brown, sky-blue, and of different degrees of intensity. The seat of these coloured vesicles is in the mucous substance, and they are covered by the epidermis, which is smooth and transparent. They have no visible connection with any vascular system, nor with the part of the body immediately below them; they are simply animated by very delicate nervous filaments, which are scarcely discernible, even by means of a microscope. The colour with which they are provided is not from a circulating fluid, nor from a contained fluid, but belongs to their structure. Our author first describes the properties of these vesicles in the dead and afterwards in the living animal.

(1.) *Appearances in the Dead Animal.*—All these coloured vesicles exhibit a kind of systole and diastole, or rather a contraction and expansion. This motion, in these vesicles, is produced when we blow on the animal, or expose it to the light, or touch it gently with the finger. During the systole the grains become extremely minute, while, during the diastole, they expand to fifty times their former size. In this state of expansion, the coloured vesicles assume the form of empty grape-husks. When these vesicles, during their diastole, have acquired their greatest degree of expansion, they open generally in the middle of the upper side, seldomer lower down, when a round hole appears. The edge of this opening appears to be surrounded by a circular muscle, which is also capable of contraction and expansion.

(2.) *Appearances in the Living Animal.*—1. When the animal is in a state of repose, the vesicles are contracted, and are not visible. 2. When the animal is excited, by being touched with the hand, or otherwise irritated, the coloured vesicles shew themselves, are instantly in motion, and appear and disappear with the velocity of lightning; sometimes like spots, which

appear on different parts of the body, and sometimes like waves, that move across its surface with the velocity of an arrow. This phenomenon is caused by the rapid and simultaneous contraction which takes place in all the vesicles of a particular part of the body, and from the sudden and simultaneous expansion of all the vesicles on another part. This appearance continues until the whole body of the animal is covered with it, and its natural colour is changed for that of the vesicles. 3. If the animal remains long in this state it dies; but if it is again returned to its proper element, and we cease to irritate it, then it begins to compose itself, considering danger as over, when the vesicles become smaller and smaller; and when they entirely disappear, the skin assumes its natural colour. From the preceding observations, the following results are obtained:—1. Each of these coloured vesicles, in their expanded state, assumes the oval or round shape; internally they are empty, and resemble the husk of a grape. 2. Each of these vesicles is composed of a skin, having the structure of felt, is very irritable, and, on account of its structure, is necessarily capable of expansion and contraction. 3. The motion of systole and diastole, in these vesicles, can be produced by a very slight irritation, even long after the death of the animal. 4. These vesicles are provided with a circular aperture, which can open and shut, probably by means of a circular muscle, and which enables us to see to the bottom. 5. The expansive and contractive power with which they are provided, during life, is owing to their particular structure, which is subjected to the influence of the general nervous system, with which they are connected by means of delicate nerves; for every motion in them ceases when the skin is separated from the body. 6. The power which the vesicles possess, of moving, after the death of the animal, depends on a remaining portion of excitability. 7. These vesicles, in the dead animal, are sensible to the action of stimuli, and are quick in their motions; in the living state they are very irritable, because their contraction and expansion take place with extraordinary rapidity. 8. The motions of these organs are voluntary during life, but passive after death. 9. The vesicles are subject, not only to a particular individual motion, but also to a general motion, which is undulatory, and is peculiar to the dermoidal system. 10. This dermoidal organ, which consists of different orders of microscopic empty vesicles, distinguish-

ed by different colours, and provided with a contractive and expansive power, occurs in no other beings of the animal kingdom, but in the tribes of the class Cephalopoda. 11. Lastly, this new organic system, which is remarkable, on account of its position, singular on account of its structure, and surprising by its phenomena, ought to be arranged by naturalists as a particular organic system. Sangiovanni names this organ, on account of its phenomena, its power of expansion and contraction, and, lastly, its position, *Systema cromofero-espansivo-dermoidale*, and considers it as an organ of defence to the animals possessing it.

Additional.—In Blainville's *Principes d'Anatomie Comparée*, vol. i. p. 198, I find the organ above described, noticed in a very general way. The following are De Blainville's observations: "C'est sur le peau d'un genre de cet ordre, celui des calmars, qui se voit un singularité de coloration fort remarquable. Les taches colorées en rouge plus ou moins vif dont elle est parsemée assez irregulièrement, sont dans un sorte de mouvement de systole et de diastole continuel; c'est-a-dire que parvenues à toute l'etendue dont elles sont susceptibles, elles diminuent peu-à-peu jusqu' à devenir presque imperceptibles, pour augmenter ensuite graduellement de nouveau, et ainsi de suite."

ARTS.

26. *Cotton-Yarn.*—Cotton-yarn has been spun of the fineness of 350 hanks weighing only one pound. Each hank would measure 840 yards, which multiplied by 350, will give 294,000 yards, or 167 miles and a fraction.—*Heywood's Discourses* *.

27. *Medical Remains found at Pompeii.*—M. Choulant has lately published, at Leipsic, in a pamphlet, entitled *De Locis Pompeianis ad Rem Medicam facientibus*, an account of the different objects relating to the medical art, which have been discovered at Pompeii. (The eruption of Vesuvius, under which the ancient city was buried, and in which the elder Pliny fell a victim to his ardour for the advancement of science, occurred about the seventy-ninth year of the Christian era). M. Choulant successively describes the Temple of Esculapius, the amulets, surgical instruments, pharmaceutical apparatus, &c., found in the midst of the ruins. Amongst the surgical instruments were

* The above notice, and some others that follow, are selected from Mr Heywood's interesting Discourses, delivered before the Royal Institution at Liverpool.

found some nearly resembling those made use of at the present day; as, for instance, elevators for the operation of trepanning, lancets, spatulæ, instruments for the application of the actual cautery, &c.* There has not been found one single building which could be regarded as a school of surgery or anatomical museum.

POLITICAL ECONOMY.

28. *Benefit Societies.*—In our last Number, (p. 169.) we noticed a work, which was then in the press, on the principles according to which Friendly Societies ought to be conducted. That work has since been published, and, from the very able manner in which the Report has been drawn up, reflects the highest credit upon Mr Oliphant, and the other gentlemen of the Committee, by whom it has been brought forward.

Since so much has thus been done for Friendly Societies in general, it remains with the members of such as now exist, to apply the rates and calculations to their own respective cases, in order to ascertain how far their funds are in such a state as to afford a fair prospect of permanency; for it has been shewn to demonstration, that unless the funds in hand, together with the future contributions of the existing members, be equivalent to the whole future allowances to the existing members (without calculating upon the admission of any new member at all) the Society is not in a solvent state: and, consequently, if they continue at the same rates of contribution and distribution, they must sooner or later fall into the same state of ruin, as the numerous Societies which have preceded them in a similar course of delusion.

We believe, that there are many Societies which are already ruined, and that almost beyond remedy; but that there are many others whose affairs may yet be brought into a sound and permanent condition, by investigation and amendment. The means of making such investigations are amply afforded by the Report; and no very high degree of arithmetical knowledge is necessary to enable the members to conduct such investigations themselves. But should they have any diffidence in their own abilities for such an undertaking, they may easily obtain the assistance of those who have devoted much of their time and attention to the subject, and who may, consequently, be considered capable of making up a distinct and accurate state of their af-

fairs. It would certainly be wiser in the members of a Society to contribute a small sum (perhaps sixpence each) to cover the expence of an investigation, than to continue in ignorance as to the real state of their affairs, when perhaps they are daily going to decay.

It is to be apprehended, however, that many of the more aged members, and such as are present burdens upon Friendly Societies, may set their faces against every such investigation; but it becomes the incumbent duty and the personal interest of the younger and more healthy members, to ascertain that, while they are giving certain allowances to their members at present in distress, an adequate fund shall remain to afford similar allowances to themselves, when they, in their turn, shall come to require them. More they should not desire; but we are at a loss to see how less should satisfy them.

The only information necessary to be transmitted to any professional gentleman, who may be consulted upon the affairs of a Friendly Society, and who is possessed of the Report to the Highland Society, or of the 6th volume of the Society's Transactions, are—

1st, The number and relative ages of the members; 2d, The amount of the funds in hand; 3d, The rates of contributions and allowances; and, 4th, A copy of the regulations of the Society.

29. *Steam-Engine.*—To eulogise the steam-engine is become common-place. Its value to this country may be estimated from calculations which shew, that the steam-engines in England represent the power of 320,000 horses, equal to 1,920,000 men, which being in fact managed by 36,000 men only, add actually to the power of our population 1,884,000 men.—*Heywood.*

30. *Cotton Manufactures.*—" The extent of the national interest in the skilful cultivation of the productive labour of this great country, is not adequately understood. I will step only on the threshold of the subject, by endeavouring to shew its importance in producing taxable capital; presuming that the whole sum received for British manufactures and produce in commerce with other nations, with the deduction paid to them for raw materials, will be allowed to be so considerable, when it is borne in mind, that, in the case of a general and equal income-tax, those profits and wages would be liable to it.—The value of the cotton manufactures exported during the twenty-two years of the

late war, from 1793 to 1815, amounted to 208 millions Sterling at the official value. The raw material, at 4 millions per annum, amounts to 88 millions Sterling. The net annual receipt from foreign countries for profits and wages, was therefore 120 millions, or about 5½ millions per annum. But the whole value of all the British manufactures exported during that period was 548 millions, which, after deducting for the raw material 148 millions, will leave 400 millions added to the taxable capital of the nation, at the rate of more than 18 millions per annum by amount received for the wages and profit of British productive labour. In the eight years since the return of peace, from 1815 to 1822, the cotton manufactures exported are upwards of 177½ millions at their official value; and, deducting 5 millions per annum for the raw material, leaves 137½ millions, being about 17¼ millions per annum, which being added to the export of the twenty-two years preceding, will make upwards of 257½ millions contributed since the commencement of the late war by cotton manufactures alone to the taxable capital of the nation. But in the last eight years, the whole amount of exported British manufactures and produce is 332 millions; and, deducting the raw materials at the increased rate of 7½ millions per annum, will leave 272 millions, being about 34 millions per annum; which being added to the produce of profit and wages for the twenty-two years of the war, as before mentioned (400 millions), will make 672 millions received in the last thirty years since 1793, being upwards of 22½ millions per annum for wages and profits produced by British industry, and received from other nations. During the war, the sum added to the national debt by loans was 569 millions, which it thus appears was exceeded upwards of 100 millions by the amount received from foreign countries for the ingenuity of the English artisan, and the industry of the English labourer."—*Heywood's Discourses.*

MISCELLANEOUS.

31. *Value of Literary Property in Scotland forty years ago.*—The late Charles Elliot, Esq. of Edinburgh, the most eminent publisher of his time, by the liberality of his transactions with authors gave a powerful stimulus to literary exertion. In proof of this, it may be mentioned, that, in April 1784, (as

appears from a letter of that date, in the handwriting of the late Dr Walker, Professor of Natural History, now before us), he paid to the celebrated Dr Cullen, for a new edition of his *First Lines*, in 4 vols. 8vo, the sum of £1200; and, in 1786, to Mr Smellie (as we learn from the son of that eminent naturalist), for the first edition of his *Philosophy of Natural History*, in one volume 4to, £1050. Such a quarto volume was equal to two octavo volumes like the First Lines of Cullen.

32. *Evening Party at M. Arago's.*—A friend who lately visited Paris, at one of M. Arago's *soirées* met with the following distinguished persons, all of them remarkable for having performed journeys or adventures of which there was no parallel. 1. There was Professor Simonoff, who was Astronomer to the Russian Expedition into the Antarctic Circle, and who had been nearest to the *South Pole* of any man living. 2. Captain Scoresby *junior*, who had been nearest to the *North Pole* of any one living. 3. Baron Humboldt, who had been higher on mountains than any other philosopher. 4. Madame Freycinet, the only lady who had ever accompanied a voyage of discovery, and circumnavigated the globe. 5. M. Gay-Lussac, who had, we believe, been the highest in the air of any man. 6. M. Callien, who had travelled with the son of the Pascha of Egypt further towards the sources of the Nile than any person now living.

Art. XXXI.—*List of Patents granted in Scotland from 19th May to 13th August 1824.*

49. To John Holt Ibbetson, of Smith Street, Chelsea, county of Middlesex, Esq. for "certain improvements in the production or manufacture of gas." Sealed at Edinburgh 21st June 1824.

50. To William Harrington of Crosshaven, county of Cork, Esq. for "an improved raft for transporting timber." Sealed at Edinburgh 21st June 1824.

51. To George Vaughan of Sheffield, county of York, for "an improvement on steam-engines, by which means power will be gained, and expence saved." Sealed at Edinburgh 26th June 1824.

52. To James Viney of Shanklen, in the Isle of Wight, Colonel in the Artillery, for "improvements in and additions to water-closets." Sealed at Edinburgh 26th June 1824.

53. To Robert Garbut of Kingston upon Hull, merchant, for "an apparatus for the more convenient filing of papers and other articles, and protecting the same from dust or damage, including improvements on or additions to the files in common use." Sealed at Edinburgh 26th June 1824.

54. To William Busk of Broad Street, London, Esq. for "certain improvements in the means or method of propelling or moving ships, boats, or other floating bodies." Sealed at Edinburgh 4th August 1824.

55. To Matthew Bush of Westham, county of Essex, calico-printer, for "certain improvements on machinery, or apparatus for printing calicoes, and other fabrics." Sealed at Edinburgh 13th August 1824.

LIST OF PLATES IN VOLUME XI.

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P. Neill, Printer.